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CONTENTS

Interference in Maze Learning as a Factorial Function of Similarity and Goal Gradient: L. S. KOGAN 69

"Spontaneous Alternation" as a Function of Time between Trials and Amount of Work: K. C. MONTGOMERY 82

Conditioned Discrimination as Related to Anxiety: E. R. HILGARD, L. V. JONES, AND S. J. KAPLAN 94

Two Variables in the Acquisition of Depressant Properties by a Stimulus: A. LIBBY 100

Drive (D) as a Function of Hours of Hunger (h): H. G. YAMAGUCHI 108

Motivation in Learning: X. Comparison of Electric Shock for Correct Turns in a Corrective and a Non-Corrective Situation: K. F. MUENZINGER AND R. F. POWLOSKI 118

Studies of Distributed Practice: I. The Influence of Intra-List Similarity in Serial Learning: B. J. UNDERWOOD AND D. GOAD 125

Effects of Total Light Flux on Critical Flicker Frequency after Frontal Lobe Lesion: W. S. BATTERSBY, M. B. BENDER, AND H. L. TEUBER 135

Sensitivity to Apparent Movement in Depth as a Function of "Property of Movement": W. M. SMITH 143

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INTERFERENCE IN MAZE LEARNING AS A FACTORIAL FUNCTION OF SIMILARITY AND GOAL GRADIENT [1]

BY LEONARD S. KOGAN

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The conditions which foster the occurrence of retroactive inhibition in maze learning have never been thoroughly explored. Whether RI can even be demonstrated in rat maze learning has been a subject of dispute. Of seven studies relevant to this problem, four (**9, 21, 35, 36**) found evidence for decrement in retention as a function of interpolated learning while three (**3, 13, 34**) presented evidence for facilitation.

One limitation of work on RI in mazes is that the learning situations have generally been complex. Conclusions have been based on analysis of total errors, total time, or total trials for mazes as a whole. Interference in some sections of a maze may be counterbalanced by facilitation in other sections and any overall inference would then be largely a net function of the two effects. Analysis of possible variations in effect within the sub-units of the maze has not been attempted.

[1] This article is adapted from a dissertation submitted to the faculty of the Department of Psychology of the University of Rochester in partial fulfillment of the requirements for the Ph.D. degree. The writer is greatly indebted to Dr. S. D. S. Spragg and Dr. I. E. Farber for their counsel during the course of the study.

Secondly, the degree of similarity between the original and interpolated activities has not been systematically controlled. Degree of similarity of tasks has for many years been regarded as one of the major determinants of RI, although "similarity" has been variously defined. In relation to maze learning, the experiments in which the interpolated materials possess varying degrees of functional identity (identical elements) with the original material are especially relevant. Several studies have involved partial identity in the form of common pathways (**35, 36**), but in none was systematic variation in degree of identity investigated. Evidence from experiments with verbal materials (**19, 22**) indicates that interference tends to increase with an increase in common elements but reaches a maximum and then decreases as identity of the two tasks is approached. The assumption seems logical that RI will occur in maze learning when the interpolated learning consists of antagonistic maze learning.

Thirdly, in none of the studies on RI in mazes was the problem of the "goal gradient" investigated. Responses nearer the goal tend to be more

rapidly learned when other factors are either eliminated or controlled (**4, 14, 30**). Hence, it seems reasonable to expect that variations in susceptibility to interference or facilitation might well be related to the position of choice points within the compound learning sequence.

The present study is an attempt to investigate the following major questions: How does the interpolation of a simple maze problem affect the retention and relearning of an original maze habit? Does interference or facilitation occur? Are such effects related to (a) the degree of objective identity of the two problems and (b) the relative position of choice points within the sequence?

Experimental Method

Apparatus.—Experimental conditions were formed by varying the sequence of choices in the maze presented diagrammatically in Fig. 1. The maze consists of three triangular units connected by straight runways. The midpoint of the base of each triangle is a R-L choice point with the correct choice determined by blocking one of the pathways near the vertex of the triangle. The maze is alley-type, made of wood covered with hardware cloth and painted a dull black. Alleys are 4 in. wide but starting 10 in. before each choice point, they narrow to 2.5 in. to eliminate centrifugal swing (**1**). The gates are of the drop type and are remotely controlled. After *S* has entered a choice arm the gate just traversed is dropped to prevent retracing. One source of light is suspended centrally above the maze in a windowless room.

Procedure.—Each *S* learned one of the eight possible patterns of choices as the original problem. There remained seven different patterns which were used for the interpolated patterns of seven experimental groups. The eighth group was made up of control *Ss* who had no interpolated problem. Thus the total number of unique OL–IL–RL sequences was 64. Table I gives in compact form the design of the study. Two rats, one male and one female, were run in each of the sequences, giving a total of 128 *Ss*. *Ss* were albinos between 60 and 90 days of age, and were assigned at random, by sex, to the 64 cells, with random replacements for those failing to achieve the criterion in the habituation period (see below).

The factorial nature of the design becomes more evident from Table II. Here the cases are grouped according to the location of the antagonistic choice responses introduced in the interpolated patterns. Groups A, B, and C were those in which a single reversal was introduced at the first, second, and third choice point. Groups AB, AC, and BC had reversals in the interpolated maze at the first and second, first and third, and second and third choice points. Group ABC had all three choices reversed. This design also provides variation in the degree of formal similarity between OL and IL with degrees of "identity" from 2 to zero as shown in Table II.

Training schedule.—Hunger deprivation was 48 hr. before the first habituation trial and approximately 20 hr. thereafter. *Ss* were fed for 20 min. at the close of each day's runs. The reward per run was a small pellet of chow presented in a cup at the far end of the goal box.

On the first three days each *S* was run for 2, 4, and 6 trials down the central straight runway with the R-L choices blocked. All *Ss* who did not run this path in 10 sec. by the twelfth habituation trial were eliminated (20 *Ss*).

Eight OL trials were run: six on Day 4 and two on Day 5. Eight trials of IL were then run: four on Day 5 and four on Day 6. The RL session consisted of a minimum of eight trials to a maximum of 12 trials on Day 6. Runs were made according to the technique of "corrective" learning whereby *S* proceeds until he reaches the goal box. Blocks of 10 to 13 *Ss* were run in

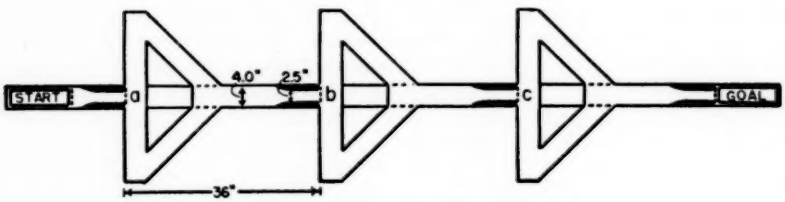

Fig. 1. Floor plan of maze. Gates are indicated by dotted lines.

TABLE I

DESIGN OF EXPERIMENT

OL and RL Patterns	IL Patterns							
	LLL	LLR	LRL	LRR	RLL	RLR	RRL	RRR
LLL	2*	2	2	2	2	2	2	2
LLR	2	2*	2	2	2	2	2	2
LRL	2	2	2*	2	2	2	2	2
LRR	2	2	2	2*	2	2	2	2
RLL	2	2	2	2	2*	2	2	2
RLR	2	2	2	2	2	2*	2	2
RRL	2	2	2	2	2	2	2*	2
RRR	2	2	2	2	2	2	2	2*

* The cases in the diagonal are controls which had no IL.

varied succession on each day with a minimum interval of 15 min. between successive trials for a given *S*. The typical interval between trials was 20–25 min. During the IL of the experimental *S*s, each control *S* received his allotment of one pellet of food when his turn occurred but remained in the carrying apparatus.

The rationale for the training schedule is as follows: Trials within a day were at least 15 min. apart in order to minimize *S*'s tendency to take varying pathways on successive trials (**11**). The shift from OL to IL and from IL to RL occurred under relatively massed conditions to provide a situation most conducive to interference (**5**). Eight trials were chosen for OL because a preliminary study had shown attainment of two or three successive errorless runs within eight trials. The same number of trials were chosen for the IL because of the theoretical proposition that maximal interference between habits tends to occur when a second habit is learned to the same degree as a first habit (**28**).

TABLE II

MAJOR GROUPS OF *S*s ACCORDING TO VARIATIONS IN IL PROBLEM

Group	Conditions during IL*	Degree of Identity	*N*
Control	...	...	16
A	−++	2	16
B	+−+	2	16
C	++−	2	16
AB	−−+	1	16
AC	−+−	1	16
BC	+−−	1	16
ABC	−−−	0	16

* + indicates no change at choice point from OL; − indicates reversal at choice point from OL.

Scoring procedure.—Several methods were used:

1. Errors were tabulated for each of the three units of the maze, as well as the total maze. Only one error was counted even if *S* made repeated entrances into the same blind alley. Entry into an incorrect arm was an error if the body of *S* penetrated completely into the arm past the choice point.

2. Time scores were kept for each unit of the maze and the total maze. The time score was the number of seconds elapsing between complete entry of *S* into one unit and complete entry into a subsequent unit.

3. In the RL sessions, the number of trials to reach three successive errorless runs for the total maze was tabulated. Maximum score was 12, given to *S*s completing three successive errorless runs on the twelfth trial and the 17 *S*s who failed to meet the criterion. The number of trials to three successive errorless choices was also tabulated for each unit of the maze and the mean of these three scores was called the mean unit criterion score.

RESULTS

The course of performance with respect to correct choice responses is presented in Fig. 2 while the trial-by-trial time scores are presented in Fig. 3.

Original Learning

Analysis of data for IL and RL assumes that the random selection of *S*s for the eight groups yielded com-

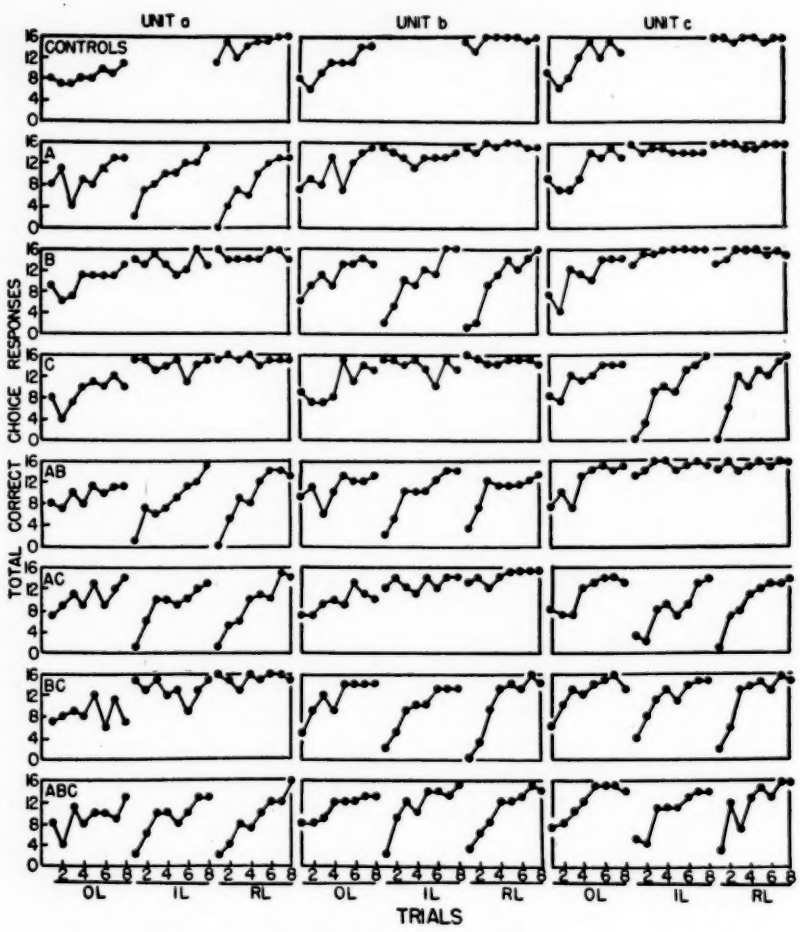

FIG. 2. Total correct choices by group for each unit of the maze on successive trials

parable OL response tendencies. To test the validity of this assumption the time and error scores for OL Trials 1–8, 2–8, and 5–8 were subjected to analyses of variance in which four major sources of variation and first-order interactions were removed from total variation. The residual was used as an estimate of error. The major sources of variation were (a) group, (b) pattern of correct choice sequence, e.g., LLL, of which there were eight, (c) units in maze, of which there were three, and (d) sex. The results of these analyses led to the following conclusions:

1. Differences between the eight groups in error and time scores during OL could be attributed to chance

variation. For example, error scores for Trials 1–8 yielded an $F = 0.4$ with 7 and 273 *df*.

2. The *S*s committed fewer errors and spent less time in each successive unit of the maze. The extent of this effect may be seen in Fig. 2 and 3 and Table III.

3. Error and time scores were significantly related to the sequence of choices to be learned and to sex.

Interpolated Learning and Relearning

In all cases the introduction of reversals during IL quickly effected a modification of choice-point behavior. Fig. 2 shows marked specificity of interference effects. In units which were reversed during IL, *S*s had to learn the new choices but this apparently did not disrupt their generally correct choices in the non-reversed units. This specificity of interference

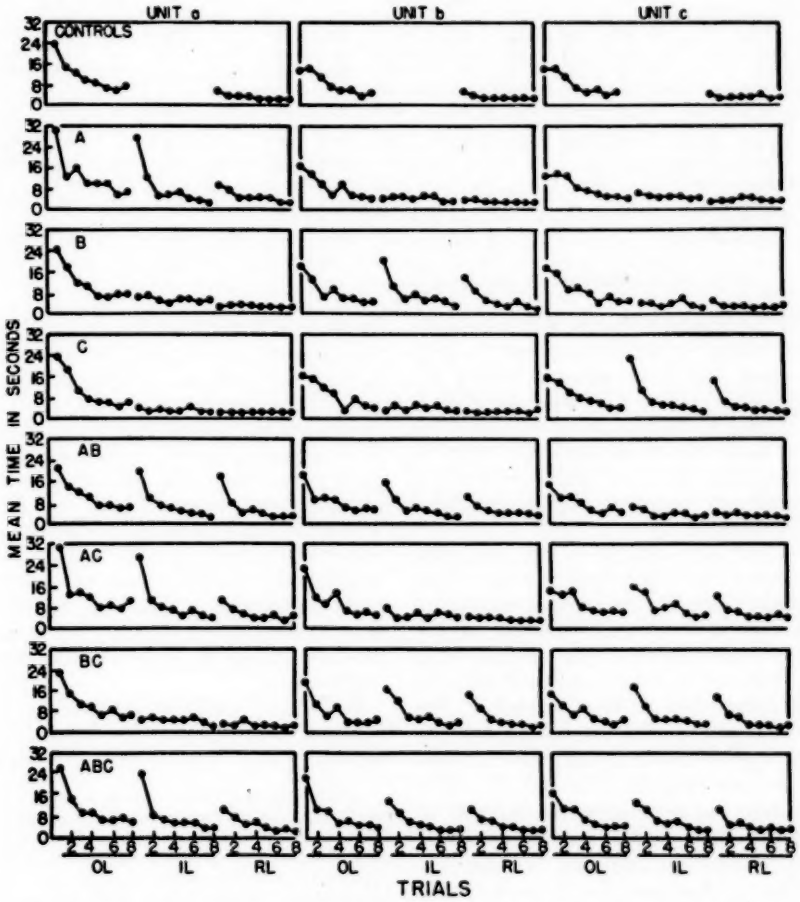

FIG. 3. Mean time scores in seconds by group for each unit of the maze on successive trials

TABLE III

PER CENT CHOICES IN ORIGINALLY CORRECT DIRECTION BY GROUP BY UNIT FOR EIGHT TRIALS OF OL, IL, AND RL

Group	OL				IL				RL			
	Unit			Total	Unit			Total	Unit			Total
	a	b	c		a	b	c		a	b	c	
Controls	53	66	70	63	—	—	—	—	89	96	98	95
A	60	66	68	65	**41***	83	91	72	**51**	95	98	82
B	62	69	67	66	84	**37**	96	72	92	**62**	95	83
C	56	66	72	65	88	86	**42**	72	95	92	**66**	84
AB	59	67	74	67	**47**	**40**	93	60	**59**	**62**	95	72
AC	66	59	69	65	**45**	80	**48**	58	**56**	88	**62**	69
BC	53	71	77	67	82	**41**	**29**	51	95	**64**	**73**	78
ABC	57	68	75	67	**44**	**30**	**35**	36	**55**	**65**	**74**	65
All exp. groups	59	67	72	66	62	57	62	60	72	76	80	76
Reversed units					**44**	**37**	**38**	**40**	**55**	**63**	**69**	**62**
Non-reversed units					85	83	93	87	93	93	96	94
Difference (main effects)					—41	—46	—55	—47	—38	—30	—27	—32

* Bold-face entries indicate units reversed during IL.

is again shown during RL, where the majority of errors occurred in units which had been reversed during IL. Similar results are seen in Fig. 3. During IL and RL, *Ss* spent more time in units where reversals had been introduced than in units where the originally correct choice remained unchanged.

Per cent choices in the originally correct direction for the combined eight trials of each learning period are shown in Table III. During IL the per cent of correct choices for the units which had not been reversed were 85, 83, and 93 in units *a*, *b*, and *c*. For the reversed units the comparable per cent of choices in the originally correct direction was 44, 37, and 38. The differences between these two sets of percentages yield an index of the effect of each unit reversal during IL These differences (−41, −46, −55) reveal that the *Ss* had the least difficulty in adjusting to the reversal in the case of unit *c*, and the greatest difficulty in unit *a*. During RL the per cent of correct responses at choice points never reversed was 93, 93, and 96 in units *a*, *b*, and *c*. The comparable percentages of correct choices in reversed units were 55, 63, and 69. The differences between reversed and non-reversed units (−38, −30, and −27) again indicate that the more rapid relearning of the original response tendencies, which had been reversed during IL, were made in the order *c*, *b*, *a*.

In Table IV is shown an analysis of variance of error scores for Trials 1–8 of RL.[2] The *F*-ratios indicate that

[2] Such analysis of variance may be criticized on the basis that the data violate assumptions of homogeneity of variance, normality, independence of means and variances, etc. Analyses of transformed scores, however, showed that the significances of effects tended to be enhanced without alteration of inferences.

TABLE IV

ANALYSIS OF VARIANCE OF ERROR SCORES DURING TRIALS 1–8 OF RELEARNING

Source of Variation	*df*	Mean Square	*F*
Group	7	28.03	15.07**
Pattern	7	3.75	2.02
Unit	2	15.49	8.33**
Sex	1	2.50	1.34
G × P	49	2.72	1.46*
G × U	14	32.50	17.47**
G × S	7	2.73	1.47
P × U	14	2.72	1.47
P × S	7	2.23	1.20
U × S	2	.04	.00
Error	273	1.86	—
Total	383	—	—

* Significant at .05 level.
** Significant at .01 level.

the total scores for the groups tended to differ and also that the effects of group differences were differential with respect to the different units of the maze. Analysis of variance for Trials 5–8 of RL indicated that the group differences persisted beyond the first four trials of RL. The presence of "significant" group differences was also indicated by similar analyses of variance for time scores and number of trials to the criterion of mastery. Traditionally, the main comparisons of the RI experiment have been between the scores of the control group

TABLE V

t-RATIOS FOR DIFFERENCES IN MEAN ERRORS DURING RELEARNING TRIALS 1–8 FOR CONTROL AND EXPERIMENTAL GROUPS

Group	Unit *a**	Unit *b*	Unit *c*	Total
A	*6.0***	0.2	0.0	*4.6***
B	−0.6	*6.5***	1.8	*3.6***
C	−1.3	1.2	*8.5***	*4.1***
AB	*4.1***	*3.5***	1.4	*4.5***
AC	*5.5***	1.6	*5.1***	*6.0***
BC	−1.7	*6.7***	*5.9***	*5.5***
ABC	*5.2***	*5.2***	*5.8***	*8.1***

* Italics indicate comparisons for units which had been reversed during IL.
** Significant at or below .01 level.

TABLE VI

MEAN ERRORS DURING RELEARNING TRIALS 1–8 AS A FUNCTION OF DEGREE OF IDENTITY BETWEEN OL AND IL PATTERNS

Degree of Identity	*N*	Mean	*SD*
0	16	8.44	3.12
1	48	6.52	3.75
2	48	4.12	2.33
Control	16	1.31	1.35

and experimental groups. Comparisons between the controls and each of the experimental groups were made for error and time scores as based on Trials 1–8 and 2–8 of RL as well as for trials to the criterion. Since all results were markedly analogous, Table V presents only the *t*-ratios based on two-group differences in mean errors during Trials 1–8 of RL. The largest differences and, in fact, the only ones which yielded *p*-values at or below .01, were those which occurred in the units which had been reversed during IL.[3]

Identity of pattern.—The comparisons of efficiency of RL as related to degree of identity of OL and IL patterns are based on consideration of total maze performance. If the effect of reversal tends to be specific to the unit reversed, it may be expected that total relearning efficiency will vary inversely with the number of reversals introduced during IL. Table VI presents total error scores for Trials 1–8 of RL for the control group and the three groups assembled according to varying degree of identity of OL and IL. There was a progressive increase in mean number of errors from

[3] In the RI paradigm the first trial of RL is often designated as the "recall" trial. The interference effects of reversals were so marked during this trial, as shown in Figs. 2 and 3, that no statistical tests are necessary to establish their significance.

the controls with 1.31 to the group which had all three units reversed (identity degree 0), which had 8.44 mean errors. These means were compared both by two-group *t*-tests and by use of the error estimate from the analysis of variance. All *p*-values fell at or below .01 except the difference between degree 1 and degree 0, as based on the two-group *t*-ratio. A similar analysis for Trials 2–8 of RL yielded smaller *t*-ratios but the increasing error trend with decreasing identity was again evident. The results for mean time scores were of the same nature.

The error scores for Trials 1–8 of IL and RL were converted to per cent of choices correct and are presented graphically in Fig. 4. Similar general trends are to be noted for both IL and RL, with mastery during RL being at a higher level of efficiency. The curves for mean time scores during IL and RL, as related to identity of pattern, are also presented in Fig. 4. Time scores similarly indicate decreasing efficiency of RL with decreasing degree of identity. Identity comparisons based on the criterion of three successive errorless runs were also carried out. The controls took a mean of 4.38 trials to reach the criterion, while the mean number of trials for degrees of identity 2, 1, and 0 were 7.79, 7.75, and 8.62. Only the differences between the controls and each of the other groups were associated with *p*-values below .01. Obviously, number of trials to reach an overall criterion does not adequately describe the choice behavior of *Ss* within the different units of the maze.

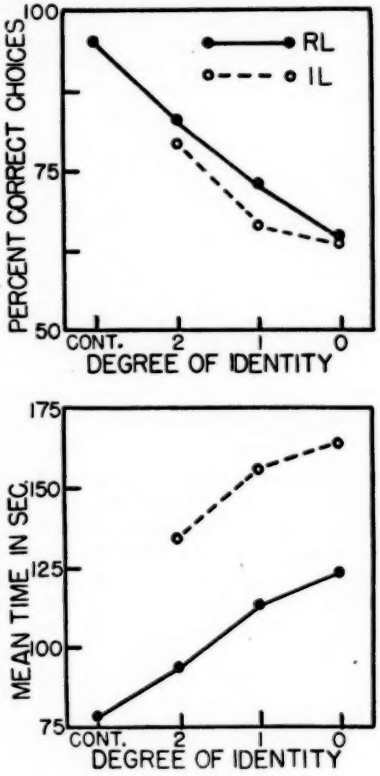

FIG. 4. Efficiency of IL and RL as a function of the degree of identity between the OL pattern and the IL pattern

Position of choice responses.—The relation between efficiency of RL and the proximity of the reversed choice point to the goal was analyzed in two ways. The first method was to compare the obtained error and time scores for all units which had been reversed during IL. For each unit there were 64 instances where reversals had to be learned. The mean error scores for Trials 1–8 of RL in reversed units were 3.6, 2.9, and 2.5 for units *a*, *b*, and *c*, and the comparable mean time scores were 46.8 sec., 43.0 sec., and 40.3 sec. Inter-unit *t*-ratios indicated that only the differences between unit *a* and unit *c* were large enough to provide *P*-values below .01. Thus, despite the higher strength of incompatible response tendency at the choice

point nearest the goal at the beginning of RL, *S*s apparently were able to master responses at this choice point most efficiently.

A more elaborate analysis was carried out taking advantage of the factorial design of the study which allows the scores of every *S* to enter into every comparison of effects (**29**). For RL this method involved the isolation of seven comparisons based on the seven *df* associated with the eight major groups in the experiment. In this factorial analysis there are three "main effects" indicated by the differences presented in the last row of Table III, three double interactions, and one triple interaction. None of the interaction terms was significant beyond the .01 level, indicating that the total effect when more than one unit is reversed is largely an additive function of specific effects in reversed units. Furthermore, tests of significance of differences between the "main effects" indicated that the *S*s in running reversed units approached the efficient performance displayed in running non-reversed units most rapidly in the unit nearest the goal.

Discussion

The main point of this study was to demonstrate the occurrence of RI in the retention of a relatively simple maze habit. Results are analogous to those of certain experiments with verbal materials. The experiments of Robinson (**26**), Kennelly (**19**) and others indicate that RI increases with decreasing percentage of formal identity between original and interpolated verbal materials. Harden (**10**) criticized this range of variation as representing only the first portion of the Skaggs-Robinson curve for the relationship between similarity and RI. Similarly, in the present study, the stages of identity covered only that portion of the curve where degree of identity could be expressed in terms of percentage of identical elements. The maximum of dissimilarity was provided by reversing the choice at all three choice points and this maximum may be compared with the condition of non-identical consonants in the two halves of the Robinson memorization lists (**26**). Since we also found that the efficiency of relearning the original maze varied directly as the number of identical elements in the original and interpolated maze, the generality of the first portion of the Skaggs-Robinson curve extends at least to simple maze learning in rats. Osgood (**25**), however, has recently discussed the untestability of the Skaggs-Robinson hypothesis and has presented a more cogent hypothesis regarding transfer and retroaction. According to this hypothesis maximal interference is to be expected when stimuli remain functionally identical and responses are antagonistic during IL. This would seem to fit the conditions in the present experiment.

The present study was not designed to present evidence for or against the classical theories of RI, e.g., perseveration, neurological transfer, transfer and disruption, etc. However, the most relevant theory appears to be some kind of transfer and disruption theory (**35**). In its modern form this theory has developed as the two-factor theory of Melton (**23**) which ascribes the decrement in relearning an original problem after interpolation of another problem to (a) the "unlearning" of the original responses during IL and (b) the competition between OL and IL responses at the time of recall.

The experiment also has some relevance to the Tolman and Hull theories of learning. According to Tolman (**32**), learning successive

choice points in a maze is accomplished by the formation of sign-gestalt-expectations or field expectancies (**33**) for each of the choice points; at a later stage of learning these separate expectancies are "telescoped" into a single expectancy for the maze as a whole. From this formulation it is difficult to predict the effects of introducing antagonistic responses. Conceivably, at the early stages of mastery, before the successive expectancies have been merged into a single integrate, the interference effects of reversals might be expected to be quite specific, with performance at non-reversed choice points not greatly affected by reversals at other choice points. If performance, however, has reached the stage where *S*s are operating in accordance with a single "cognitive map" for the entire maze, it would seem logical to expect the entire sequence of responses to be disrupted regardless of the number of choice point reversals in the interpolated maze. Since interference effects were so specific in the present experiment, the Tolman theorist is forced to reason *ex post facto* that the degree of integration of the maze habit at the end of OL and IL was low, or else reconstruct his theory. Our results, however, are similar to those of Gengerelli (**7**) in a still simpler two-choice situation, and our maze is much simpler than many to which the Tolman theory has been applied and in which the formation of a unitary expectancy should be even more difficult to achieve through training. Although our results would seem to apply as criticisms of the Tolman theory, only experimental studies of the same maze with higher degrees of OL and IL will serve as a final test. Tolman's recent analysis of learning admits the relevance of motivation but denies any direct influencing force to the proximity of reinforcement except in terms of "vividness" (**33**). Hence the finding of the present experiment that responses nearer the goal tend to be more labile cannot be directly predicted from his theory.

The theoretical interpretation of maze learning by Hull (**14, 15, 16, 17**) likewise does not contain specific deductions concerned directly with the effects of interpolating incompatible responses upon the relearning of an original maze habit. Hull's position is that detailed quantitative statements regarding the dynamics of incompatible response potentials are as yet unwarranted in view of the meagre experimental evidence available (**16**, p. 341). Nevertheless, an attempt will be made to formulate the results of the present study along Hullian lines without, however, the presumptuousness of quantification.

Hull's basic assumption is that new S-R bonds are formed only in proximity to a reinforcing state of affairs (**18**). Among the factors determining the magnitude of "habit" increment per reinforcement is the length of time elapsing between the occurrence of the reaction and the reinforcement. This factor is called the "gradient of reinforcement" and this gradient blended with the principle of secondary reinforcement is alleged to generate the "goal gradient" phenomenon. Spence (**31**) has especially emphasized the role of secondary reinforcement in maze learning.

Hull regards the learning of a maze to be a "compound selective learning" situation where *S* works his way to the goal by "trial-and-error" on the first trial (**16**, p. 95). According to the postulate of primary reinforcement, the S-R bonds closest to the goal reaction are acquired first and these in turn acquire secondary reinforcing power for bonds further from the goal

reaction. Alleys at greater distances from the goal involve less relative difference in delay between correct and incorrect pathways, contributing to the earlier elimination of blinds closer to the goal.

In addition, Hull conceives that an important mechanism in the elimination of goal-pointing blinds may be the inhibition developed as a result of frustration in the cul-de-sac. Hull does not say whether this inhibition in culs-de-sac is also related to the temporal distance of the reaction from the reinforcement, or whether it is a constant, other factors such as depth and direction of blind being controlled. It is conceivable that frustration in a blind will be greater the closer the blind to the goal (after the first trial), and hence the increment of inhibition may be greater for such blinds than for those farther from the goal. However, since the reinforcement is also closer for blinds nearer the goal, any such stronger frustration effects would be counteracted to a greater extent.

Hull has also stressed the importance of stimulus generalization gradients in determining the distribution of errors in the maze (**17**). In our experiment the influence of perseverative and anticipatory gradients was controlled by distributing the patterns of choices within each of the eight major groups so that *in toto* just as many right-hand choices were correct as left-hand choices both prior to and subsequent to any choice point.

During OL both error and time scores tended to be smaller in the unit closer to the goal. Because of the maze design and the factorial allocation of patterns presented to the *S*s, these results reflect the influence of the goal gradient more or less uncomplicated by other factors such as goal orientation, anticipatory tendencies, choice-point expectancies, etc.

During IL reversals were mastered with less difficulty the nearer they were to the goal. Inasmuch as every incorrect response is followed by a correct choice during each trial the *S*s immediately begin to "unlearn" the old responses at each choice point and learn to take the more direct pathway. If extinction effects were to progress from the goal outward (**12, 24, 27**), learning and extinction would be working together and the regressive order of learning the new choices would be marked. If the per-trial increment of inhibition were a constant for all culs-de-sac the regressive order of learning new choices would still be in evidence. If the order of extinction were in accordance with simple habit strength (**20**) the regressive order of learning should be evident unless the gradient of extinction were equal to or steeper than the gradient of reinforcement. Regardless, however, of the form of the hypothetical extinction curve, the decreasing relative difference in the latency of delay between correct and incorrect pathways as one progresses outward from the goal would seem to insure the regressive order of learning the new choice.

During RL the *S*s again encounter some reversals and again the order of reestablishing the originally learned choices shows a goal gradient. The effect presumably results from the same factors mentioned in the interpretation of the goal gradient in IL. Further factors may be postulated to explain the higher level of efficiency of RL over IL: (a) the possible retention of some tendency to take the original choice; (b) the further practice with non-reversed choice points; (c) specific previous experience with reversals; (d) increased general familiarity with maze running; and (e) the possible increased motivation during

RL as a cumulative effect of the hunger regimen.

In accordance with the choice of a goal gradient reinforcement interpretation of results over other interpretations such as entrance gradient (**6**), differential age of response (**2**), generalization (**8**), etc., the present study appears to warrant the following generalization: Adaptive or problem-solving S-R bonds are more labile the closer they occur to a reinforcing state of affairs.

Summary and Conclusions

This study determined the retroactive interference effects of interpolated maze patterns upon the retention of originally learned maze patterns as a function of the number and sequential position of antagonistic choices introduced during interpolated learning.

Within a sample of 128 albino rats a three choice-point maze was learned in all possible original learning patterns. Interpolated learning was varied systematically from original learning by reversing 1, 2, or 3 of the choice-point directions in all combinations. Original and interpolated learning consisted of eight trials; relearning consisted of a minimum of eight trials. Learning was by the "corrective" technique, with food incentive.

During original learning the three units of the maze were learned in accordance with the expectation based on the goal gradient hypothesis.

The introduction of antagonistic responses during interpolated learning resulted in marked interference during both interpolated learning and relearning. This interference was largely "specific" to the unit(s) in which reversal occurred. As a consequence of this specificity, the efficiency of relearning the original patterns was found to be directly related to the number of reversals introduced during interpolated learning. This finding was pointed out as analogous to results found in verbal retroactive inhibition studies with functionally identical stimulus elements and antagonistic responses.

Reversals in interpolated learning and relearning were mastered more readily, the nearer the reversed unit to the goal. For this reason, it is suggested that the goal gradient hypothesis may be generalized to assert that adaptive stimulus-response connections are more labile the closer they occur to a point of reinforcement.

An attempt was made to interpret the acquisition of new response tendencies at reversed choice points during interpolated learning and relearning as based on the interaction of reinforcement of the "new" response and the extinction of the "old" response.

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"SPONTANEOUS ALTERNATION" AS A FUNCTION OF TIME BETWEEN TRIALS AND AMOUNT OF WORK

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When rats are given immediately consecutive runs in a two-choice maze with equivalent reinforcing conditions in both arms, they tend to alternate choices above chance. This simple ordering of behavior, called "spontaneous alternation," poses a problem that has been studied by a number of investigators (e.g., **1**, **2**, **4**, **9**, **14**) but that has not yet been satisfactorily solved.

The present paper reports two experiments designed to determine whether Hull's constructs of reactive inhibition and conditioned inhibition provide an adequate basis for explaining alternation behavior.[3] Hull's eighth postulate (**3**) asserts that: the performance of a response creates a negative drive state, reactive inhibition (I_R), which mediates against the repetition of that response; the amount of I_R generated is an increasing function of the amount of work involved in the response; and I_R dissipates according to a decay function of time. His ninth postulate (**3**) states that: stimuli closely associated in time with the cessation of a response are conditioned to I_R, thereby generating conditioned inhibition ($_sI_R$); $_sI_R$ summates with I_R to produce net inhibitory potential ($\dot{I}_R$); and response strength is a function of reaction potential ($_sE_R$) minus $\dot{I}_R$, i.e., of effective reaction potential ($_s\bar{E}_R$).

From these two postulates and others in Hull's system one can generate a number of theorems (predictions) applicable to those situations in which spontaneous alternation occurs. For example, rats given successive trials in a **T**-maze containing reward in both arms should tend to avoid each previous choice because of the reactive and conditioned inhibition generated by the previous turn-response. Further, they should tend to alternate less as time between choices increases or as work per response decreases because both I_R and $_sI_R$ will decrease in amount with increasing inter-trial interval or decreasing amount of work per response.

There are three important independent variables in these two postulates: number of responses, amount of work per response, and time between responses. A survey of the literature reveals only three experiments in which alternation has been related to these variables. Wingfield and Dennis (**16**) obtained over 90 per cent alternation when rats were given two trials per day in a simple **Y**-maze but only 68 per cent when they were given six trials per day. Mowrer and Jones (**9**) extinguished a simple bar-pressing response in rats placed in a situation where alternation between two bars was possible, and found that alternation was *less* for *greater* amounts of work. Heathers (**2**) demonstrated that per cent alternation is a decreasing function of inter-trial interval. The results of the first and third of

[1] This paper is based in part upon a dissertation submitted in partial fulfillment of the requirements for the Ph.D. at the University of Chicago. The writer wishes to express his gratitude to Dr. W. D. Neff.

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[3] This possibility has been suggested in several recent papers, e.g., Solomon (**11**).

these studies agree with predictions based upon Hull's eighth and ninth postulates, but those of the Mowrer-Jones study are in direct contradiction.[4] Moreover, in none of these experiments were the *simultaneous* effects of all three, or even two, of the important independent variables in these postulates investigated *in the same situation.*

This paper reports the results of two experiments designed to provide more comprehensive data relating to the phenomena of spontaneous alternation. The purpose of these studies is twofold: (1) to investigate alternation in a situation where work and inter-trial interval are varied simultaneously, and (2) to attempt to determine whether Hull's postulates VIII and IX provide an adequate basis for explaining alternation behavior. Data concerning the relation between number of trials and alternation are also provided.

The experimental situation chosen to realize these aims had to be one in which work and inter-trial interval could be varied without introducing undesired variables, e.g., delay in reinforcement. A single-unit **Y**-maze with a lever at the end of each goal-arm was used. Work was varied by counterbalancing the bars with different weights; inter-trial interval was easily controlled; and alternation between the two maze-arms was possible.

After the experiments had been completed the author realized that results obtained by this method of varying work do not provide an adequate test of predictions about alternation based on Hull's reactive-inhibition postulate: alternation is defined in terms of turn-responses, but work was varied for bar-responses. This is an important point, and it will be treated more fully in a later section.

[4] In a recent study of the alternation of guesses, Solomon (12) obtained negative results for both the time variable and the effort variable.

Experimental Design

In Experiment I a simple factorial design was used. Five rats were assigned at random to each of the nine experimental conditions obtained when three values of bar-weight—15, 40, and 90 gm.—are combined with three of inter-trial interval—20, 45, and 90 sec. In Experiment II, designed to check on Experiment I and to round out the data, five rats were assigned at random to each of three conditions—15, 40, and 90 gm. of bar-weight all at a 10-sec. interval—and eight to the condition of 40 gm. of bar-weight and a 30-sec. inter-trial interval. The conditions for each group were held constant throughout the experiments.

Procedure

The *S*s, apparatus, and procedural details of Experiment I are described in a previous paper (5) which reports the latency data obtained in this experiment. The *S*s in Experiment II were homogeneous with those of Experiment I and were run in the same apparatus under the same procedure. Each *S* was given 100 acquisition and 40 extinction trials in the **Y**-maze. On each trial *S* was allowed to enter either arm. A trial ended when *S* pressed the bar with the requisite amount of force. All trials were reinforced during acquisition, none during extinction.

Results

The data to be analyzed consist of the sequences of R and L choices made by each *S* during the 100 acquisition and 40 extinction trials. These sequences are analyzed in terms of the frequency of occurrence of two ordered pairs of choices—RL and LR. The experimental design makes it possible to compare groups of *S*s with respect to the effect on alternation of turns of (1) amount of bar-weight, (2) length of inter-trial interval, and (3) number of trials.

Acquisition.—In Fig. 1, per cent alternation during the 100 acquisition trials is plotted as a function of bar-weight and inter-trial interval. These

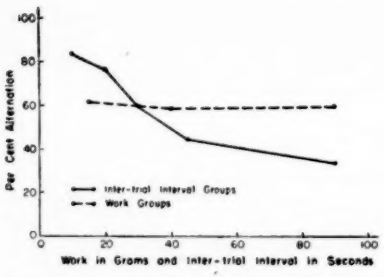

FIG. 1. Per cent alternation as a function of work and of inter-trial interval

graphs show that per cent alternation is *not* related to bar-weight, but that it is a negatively accelerated decreasing function of inter-trial interval.

To analyze these data the total frequency of alternation for each *S* was converted to per cent, the percentage scores were transformed into angles according to the formula,

$$\text{Angle} = \text{arc sin } \sqrt{\text{percentage}},$$

and an analysis of variance was performed on the transformed scores (**10**, pp. 447–450). The *F* for inter-trial interval is significant at well beyond the .001 level; the *F*s for bar-weight and interaction are well below the .05 level.

If each choice were an independent event one would predict that any given *S* would alternate 50 per cent of the time. On the basis of this hypothesis the .05 fiducial limits for a block of 20 trials were computed for a single *S*. A tabulation was made of the total number of times that the *S*s in each group alternated above and below these limits. For the 10-, 20-, 30-, 45-, and 90-sec. groups the "above" totals are 61, 48, 11, 5, and 2, and the "below" totals are 0, 0, 2, 21, and 32; for the 15-, 40-, and 90-gm. groups these totals are 41, 40, and 35, and 16, 20, and 17. It is evident that the same relations obtain as in the previous analysis.

In Fig. 2 per cent alternation is plotted as a function of blocks of ten trials for each of the three work groups. The curves begin at about the same level and drop slowly as the number of reinforced trials increases. No systematic differences among groups are apparent. The gradual decline in per cent alternation is attributable to the influence of the 30-, 45-, and 90-sec. inter-trial interval groups (see Fig. 3).

Similar curves for the five inter-trial interval groups are presented in Fig. 3. The changes in per cent alternation with increasing number of reinforced trials are striking. These curves show that per cent alternation is a negatively accelerated increasing function of number of reinforced trials for the 10- and 20-sec. groups but a negatively accelerated decreasing function for the 30-, 45-, and 90-sec. groups, dropping more rapidly as the inter-trial interval increases from 30 to 90 sec.

To analyze these data the per cent alternation for each block of 20 trials was obtained for each *S*, the percentage scores for the first, third, and fifth blocks of 20 trials were converted into angles according to the formula previ-

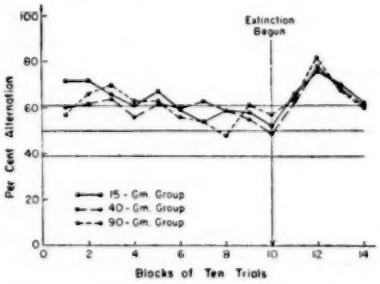

FIG. 2. Per cent alternation as a function of blocks of ten trials for the three bar-weight groups

ously described, and three analyses of variance were performed on these sets of scores. No significant differences in alternation among inter-trial interval groups occurred during the first 20 trials; marked differences appeared by the third block of 20 trials, the *F* for this comparison being significant at well beyond the .001 level; this trend continued throughout the acquisition series; and no differences among groups were produced by bar-weight at any time during acquisition.

In Figs. 2 and 3 the .01 fiducial limits of chance alternation are represented by the horizontal lines at 39 and 61 per cent (the corresponding values for the 30-sec. 40-gm. group are 36 and 64). The curves in Fig. 2 are not discussed because alternation is not related to bar-weight. In Fig. 3 it is clear that the 10- and 20-sec. groups alternated above chance throughout the acquisition series, that the 30-sec. group dropped from above to within chance limits, and that the 45- and 90-sec. groups dropped from within to below chance limits, the latter more rapidly. The behavior of the individual *S*s in these groups was consistent with the group trends. The changes in per cent alternation between the first and tenth blocks of ten trials are significant at well beyond the .01 level for all five inter-trial interval groups.

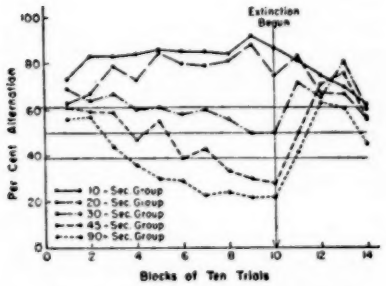

FIG. 3. Per cent alternation as a function of blocks of ten trials for the five inter-trial interval groups

TABLE I

PER CENT ALTERNATION FOR THE FIRST AND SECOND FIVE TRIALS OF ALL BLOCKS OF TEN TRIALS DURING ACQUISITION

Trials	Work (in gm.)			Inter-trial Interval (in sec.)				
	15	40	90	10	20	30	45	90
First 5	62	59	61	84	78	62	47	33
Second 5	62	59	58	85	76	57	44	36

To determine whether alternation is affected by the number of *immediately* consecutive trials the following analysis was made. The total number of alternations made by each *S* was tabulated for the first five and the second five trials in each of the ten blocks of ten trials. These values were summed and converted to percentages for each of the experimental groups. A summary of the resulting scores appears in Table I. These data show that alternation neither increased nor decreased during blocks of ten trials for any of the groups. The change in per cent alternation with increasing number of reinforced trials, therefore, is not a short-range effect.

Analysis of the frequency of alternation between the last trial of one block of ten trials and the first trial of the next yields results similar to those presented above. Per cent alternation is not related to bar-weight, and decreases from about chance for the 10-, 20-, and 30-sec. groups to significantly below chance for the 45- and 90-sec. groups (because of the development of position habits).

The results of the above analyses may now be summarized. During acquisition: (*a*) per cent alternation is a negatively accelerated decreasing

function of inter-trial interval but is not related to bar-weight; (*b*) per cent alternation is a negatively accelerated increasing function of number of reinforced trials when inter-trial interval is short (less than 30 sec.) and a decreasing function when it is 30 sec. or longer; (*c*) per cent alternation is not affected by number of immediately preceding trials; and (*d*) these relations hold whether per cent alternation per group or number of *Ss* alternating per group is considered.

Extinction.—The main results appear in Figs. 2 and 3. These graphs show that number of non-reinforced trials affects per cent alternation markedly. All groups except the 10-sec. exhibit an initial increase followed by a decrease toward chance. As in the acquisition series, bar-weight produces no differential effects, but differences among inter-trial interval groups are apparent.

A series of *t*-tests shows that the initial increases in per cent alternation are significant at beyond the .01 level for the 30-, 45-, and 90-sec. groups. The results of an analysis of variance, performed on scores transformed as before, reveal that during the last block of 20 trials there were no significant differences (.05 level) among the groups. Additional analyses, not presented here, of the number of *Ss* per group alternating above and below chance limits yield similar results. Data available from some *Ss* run an additional 20 trials show that alternation continues at a chance level for all groups.

These results may now be summarized. During extinction: (*a*) the initial effect is an increase in per cent alternation for the 30-, 45-, and 90-sec. groups and a leveling off or decrease for the 10- and 20-sec. groups; (*b*) per cent alternation bears no relation to bar-weight; and (*c*) per cent alternation approaches chance for all groups as number of non-reinforced trials increases.

TABLE II

TOTAL FREQUENCY, PER CENT, AND *P*-VALUE FOR RIGHT-GOING RESPONSES ON EACH DAY OF ACQUISITION *

Measure of Response Tendency	Day					Total
	1	2	3	4	5	
Frequency	698	707	681	714	687	3,487
Per cent	51.3	52.0	50.1	52.5	50.5	51.3
P-value	0.25	0.13	0.40	0.07	0.37	0.04

* The total frequency possible on any one day was 1,360. The *p*-values were calculated from the formula, $S.E._{prop.} = \sqrt{pq/N}$.

Controls.—No systematic differences among groups in body-weight, emotionality, or motivation were detectable (**5**). If any cues in the situation favored one goal-arm over the other, or if the task on one side differed from that on the other, either or both of two results would be expected: the total number of responses to the favored side would exceed chance, or more *Ss* would develop position habits to that side. Table II presents the frequency, per cent, and *p*-value for the number of R choices made by all *Ss* on each of the five days of acquisition. In no case is there a departure from chance. If "position habit" is defined as a frequency of alternation less than the .05 fiducial limit based on chance, then by the fifth day 17 *Ss* exhibited position habits. Eight of these were left-going, nine right-going. Thus it seems safe to conclude that there were no factors in the situation favoring one goal-arm over the other.

DISCUSSION

The present results show unequivocally that in the present experimental situation per cent alternation is a function of both inter-trial interval and

number of trials (reinforced or non-reinforced), but that it is *not* related to bar-weight. Can these findings be explained by Hull's eighth and ninth postulates?

According to the eighth postulate (3) each response evocation generates a certain amount of reactive inhibition, I_R, which dissipates as a decay function of time. If the response occurs again before I_R dissipates completely, an increment is added to the amount remaining. If, in a series of responses, the inter-response interval is greater than zero but short enough so that I_R never completely dissipates, then the amount of I_R present at the *beginning* of each response will be a negatively accelerated increasing function of number of responses. This analysis assumes a constant inter-response interval and a constant amount of work per response. As length of inter-trial interval is increased or as amount of work per response is decreased, the rate of accumulation of I_R decreases. Thus, within each block of ten trials in the present experiment differential amounts of I_R should have been generated in the various groups.

In Hull's system, probability of response evocation is a positive function of effective reaction potential (${}_s\bar{E}_R$), which equals ${}_sE_R$ $(D \times {}_sH_R)$ minus inhibitory potential $(I_R + {}_sI_R)$. Therefore, given a set of conditions generating the same amount of ${}_sE_R$ but different amounts of I_R, probability of response should vary with I_R, i.e., it should increase with inter-trial interval and decrease with work. Presumably such a set of conditions obtained in the present experiment: motivation was held constant for all groups; the variables affecting habit strength were equivalent for all groups; and the variables affecting I_R were varied from group to group.

Thus far the case involving one response has been considered. Suppose that two equally reinforced responses, *R-1* and *R-2*, are possible and that the stimuli evoking each are so similar that a large amount of stimulus generalization occurs—a condition met in the present study. If *R-1* occurs on the first trial, *R-2* will occur on the second, *R-3* on the third, and so on, because of the differences between ${}_s\bar{E}_R$*-1* and ${}_s\bar{E}_R$*-2* at the beginning of each trial produced by the relative amounts of I_R*-1* and I_R*-2*. In a series of such trials, the accumulation of I_R for each response will approach a maximum, but the amount of I_R at the beginning of any trial will always be different for the two responses provided that inter-trial interval and work are equivalent for those responses and constant over the series of trials. However, because the probability that alternation will occur must depend upon the magnitude of the *absolute* difference between ${}_s\bar{E}_R$*-1* and ${}_s\bar{E}_R$*-2*, per cent alternation should increase with work, approaching 100 as an upper limit, and decrease with inter-trial interval, approaching 50 as a lower limit. Although other considerations, e.g., increase in habit strength with number of reinforced trials, lead to certain modifications of these two predictions, a glance at the results of this experiment reveals that although alternation does decrease with inter-trial interval, it is *not* related to work (bar-weight). Therefore, in view of the fact that experimental conditions seem to satisfy those specified in the foregoing analysis, it would seem that the present results cannot be accounted for on the basis of Hull's reactive-inhibition postulate.

However, because the latency data of Experiment I (5) tend to confirm this postulate, it is necessary to in-

quire why the alternation data do not.[5] A careful consideration of the task set the *Ss* reveals an important fact. *Two different classes of responses occurred in the maze-situation: turns, and bar-presses.* Inspection of the data shows that time between bar-presses was about the same as time between turns for any one group but varied among groups according to the experimental conditions. However, although amount of work per bar-response was varied among groups, *the amount of work involved in the turn-responses must have been relatively constant for all groups.* Now, because I_R is stated to be specific to a particular response and not related to stimuli in any way, and because alternation is defined in terms of turn-responses, there is no reason to expect the amount of work involved in bar-pressing to affect the choice of turn, whereas there is reason to expect time between trials to do so. Thus, alternation should decrease with inter-trial interval but remain relatively constant with bar-weight, as was found.

Although the foregoing analysis seems straightforward enough, Thompson (**13**) has reported results which indicate that it is not complete. He ran four groups of rats in a simple **T**-maze containing a lever in each goal-arm. For any one group the bars were counterweighted in a 3:2 ratio, but the absolute values of the counterweights were varied among groups. Free and forced trials were given each day with at least 15 min. between the former. Most *Ss* learned to choose the side involving less work, as measured by choices on free trials; those *Ss* required to press greater amounts of absolute weight learned faster. Thompson interprets his results in terms of reactive inhibition. This explanation cannot be accepted. Available evidence (**11**) indicates that I_R dissipates in a short time—certainly within 15 min.; and no differential amounts of work were involved in *turning* R or L, either within or among groups. To explain these results within the framework of the Hullian system, one must invoke the ninth postulate in which the concept of conditioned inhibition ($_sI_R$) is set forth. The following discussion relates to both the Thompson and the present experiments.

In a previous paper (**5**) the mechanism of $_sI_R$ generation was explicated. Briefly, stimuli present during I_R dissipation become conditioned to the response of "resting," thus setting up $_sI_R$; the amount of $_sI_R$ thus generated is an increasing function of the amount of work involved in the response and a decreasing function of time between responses. Because $_sI_R$ is said to possess all of the characteristics of an ordinary habit, in the present situation it would generalize backwards through the maze to the choice point. Probability of response evocation bears the same kind of relation to $_sI_R$ as to I_R; hence, similar predictions result about alternation and about choosing the side involving less work. Thompson's results can be accounted for in these terms: greater amounts of $_sI_R$ were generated by greater amounts of bar-weight; the inhibitory habits generalized backwards through the maze to the choice point; thus, the *Ss* came to choose the side involving less work. This process should have occurred in the present experiment,

[5] Analysis of the latency data of Experiment I reveals that response latency increases with bar-weight and decreases with inter-trial interval, thus confirming several predictions based on Hull's eighth and ninth postulates. In contrast to the alternation results, bar-weight produced larger inter-group differences in latency than did inter-trial interval.

particularly in the 90-gm. group run at 10-sec. (a bar-weight greater by 30 gm. than any used by Thompson), and should have affected choice-behavior fairly rapidly because of the small dimensions of the maze (9-in. goal-arms). Therefore, the lack of a positive relation between per cent alternation and bar-weight must be interpreted either as disproving Hull's ninth postulate or as reflecting the operation of another and more influential factor.

The results of later experimentation (**6, 7, 8**) suggest the following hypothesis: rats run in a two-choice maze tend to alternate R and L choices because each arm evokes an "exploratory tendency" that is temporarily reduced in strength by the traversal of a given pathway but that recovers with time of non-exposure. Exploratory tendency, then, is the factor that overrides any effects produced by conditioned inhibition as far as alternation is concerned.[6]

A question arises. Does alternation result from differential amounts of I_R or from differential amounts of exploratory tendency? It is to be noted that the former implies *response-avoidance*, the latter *place-avoidance*. In the present experiments there is no way to decide between these alternatives: if an *S* avoids its previous response (turn), it also avoids the place (maze-arm) previously traversed. However, data reported by several investigators (e.g., **1, 16**) strongly suggest that spontaneous alternation can be explained more satisfactorily by the hypothesis of exploratory tendencies.

Wingfield and Dennis (**16**) gave rats four consecutive runs per day on an elevated maze where four equally long pathways led to food. The maze was constructed from two hollow rectangular units, a straight path that connected the center of one side of one unit to the center of one side of the other, and a short starting path that joined the connecting path at its midpoint. Thus a path to food required one of these four sequences of turns: RRLL, LLRR, RLRR, or LRLL. On the average, the *S*s chose three or four paths (out of four) 92 per cent of the time, and four paths 43 per cent of the time. It is difficult to see how these results can be explained in terms of reactive inhibition, but quite easy to see the applicability of the hypothesis of exploratory tendencies.

Dennis (**1**) gave 20 rats 24 pairs of reinforced trials on an elevated maze consisting of two hollow-square units connected in tandem. His *S*s alternated paths 75 per cent of the time in the first unit and 72 per cent in the second, but they alternated from one side of the maze to the other only 50 per cent between units. These results are easily interpretable in terms of the hypothesis of exploratory tendencies. But a consideration of the sequences of turns involved makes it difficult to explain them in terms of reactive inhibition: not only is the high per cent alternation within units hard to account for, but also the 50 per cent alternation between units indicates no tendency toward avoidance of turns.

A further consideration that makes the concept of I_R of doubtful value for explaining alternation behavior is that the amount of inhibition generated by

[6] Weitz and Wakeman (**15**) have suggested that "punishment" may account for spontaneous alternation. Insofar as more emotional rats are more susceptible to punishment derived from handling, the present results do not support this contention. The correlation between emotionality, as measured by amount of urine and number of feces evacuated during preliminary training, and frequency of alternation during acquisition does not differ from zero.

an R or L turning-response must be relatively small as compared to the amount generated by on-going locomotor responses, *particularly in large mazes*. Consequently, its effects on choice-behavior should be slight and difficult to measure. In many studies (e.g., **1, 16**) per cent alternation was very large in mazes of large dimensions. Therefore, the operation of another more influential factor is indicated.

In the light of the above considerations it is concluded that spontaneous alternation is explained more satisfactorily by the hypothesis of exploratory tendencies than by Hull's eighth and ninth postulates. When certain other considerations are taken into account, the results of the present experiments can be explained in terms of the former hypothesis.

The decrease in per cent alternation with increasing inter-trial interval is explained by the relation between strength of exploratory tendency and time of non-exposure. If the exploratory tendencies evoked by the two goal-arms are equal initially, and if a decrement in one is produced by the traversal of one arm, the other arm will be chosen on the next trial, the first arm on the third trial, and so on. As the time between choices is increased, the tendencies to explore the two arms will approach each other in magnitude owing to their recovery in strength with time; hence, alternation will decrease with increasing inter-trial interval.

According to this hypothesis the choice of a path depends upon how recently it has been traversed, not upon the amount of work done after the choice is made. Thus there is no reason to expect alternation to be related to bar-weight. This is not to say, however, that reactive and conditioned inhibition are never influential factors in behavior (e.g., **5**).

The relation between alternation and number of reinforced trials depends upon the parameter of inter-trial interval. To explain these results two additional factors must be taken into account. First, when a given choice is reinforced, a tendency to repeat that choice is generated. The strength of this tendency inceases with number of reinforcements, and, because of the similarity of the stimuli involved, generalizes from one maze-arm to the other. Therefore, as number of reinforced trials increases, exploratory tendency exerts less influence upon choice-behavior. Second, when that point is reached at which exploratory tendency is negligible as a factor determining choice, a state of unstable equilibrium exists. Both arms of the maze are equally "attractive," and a random sequence of turns occurs. In such a sequence a "run" of choices of the same arm will happen sooner or later. The net effect of such a run is to produce a stronger tendency to enter that particular arm; thus, a position habit develops. This state of unstable equilibrium would appear earliest in the 90-sec. group; hence, the more rapid drop in per cent alternation manifested by this group is explained.

The increasing amount of alternation with increasing number of reinforced trials exhibited by the 10- and 20-sec. groups can be explained. During the early trials the *S*s alternated significantly above chance because of the large differences in exploratory tendency produced by the short inter-trial interval. Moreover, the time between trials was short enough so that part of the stimulus situation at the time of choice consisted of proprioceptive traces derived from

the preceding turn. Thus the response of turning R was conditioned to the proprioceptive stimuli persisting from turning L, and vice versa. This, together with the fact that differences in exploratory tendency were effective over a greater number of trials, accounts for the increase in alternation exhibited by the 10- and 20-sec. groups.

During extinction new trends occurred: the 10- and 20-sec. groups exhibited a gradual decline in per cent alternation, the 30-, 45-, and 90-sec. groups an initial increase followed by a decrease to chance. It is plausible that strength of exploratory tendency decreases with number of exposures to the same situation, so that after 100 trials in a maze of such small dimensions its influence would be relatively small. Therefore, the changes in alternation during extinction must be explained in terms of decrements in the reinforcement-generated tendencies. It is an empirical fact that extinction proceeds more slowly as number of non-reinforced trials increases. Large initial decrements in the tendency to enter either arm would result in the breaking of position habits and in a sharp increase in alternation (Fig. 2 and 3). The absence of such increases in the 10- and 20-sec. groups probably resulted from their high levels of alternation which made further increases improbable, and from the extinction of responses conditioned to stimulation persisting from the previous trial. As the decrements in response strength decrease in magnitude the net differences between the tendencies to enter the two arms decrease; hence, alternation approaches chance for all groups. The fact that the 10- and 20-sec. groups approach chance bears out the contention that strength of exploratory tendency is small after 100 trials, as does the fact that those rats run an additional 20 trials continued to alternate at a chance level.

The per cent alternation obtained in the present experiments is less than that reported by other investigators. For example, Heathers (**2**) found that rats run at a 120-sec. inter-trial interval alternated about 66 per cent of the time. This value is undoubtedly significantly greater than the 34 per cent obtained for the 90-sec. group of this study. The hypothesis of exploratory tendencies suggests an explanation of this discrepancy. Strength of exploratory tendency is probably directly related to the number of stimuli (size of area) available for exploration. Heathers used an elevated maze with 30-inch arms. Thus, not only was his maze between three and four times as large as the one used in the present experiments, but also his *S*s were exposed to more extra-maze stimuli. Therefore, a higher per cent alternation would be predicted for his situation.

The exploratory-tendency hypothesis also provides an explanation of the failure of Mowrer and Jones (**9**) to obtain alternation between two levers during either acquisition or extinction. Because the number of stimuli presented by a lever is small, and because their rats were exposed to the same environmental conditions (Miller-Mowrer box) day after day, exploratory tendency must have played a minor and decreasingly important role, particularly as compared to the maze situation where *S* produces, via his own ambulatory activity, a succession of relatively novel stimuli on each trial.

The hypothesis of exploratory tendencies, then, serves to explain not only the results of the present experiment

but also those of related studies. A more explicit operational definition of the term "exploratory tendency" is required before its applicability to the problem of spontaneous alternation in general can be evaluated. Such a definition has been formulated (**6**, **7**, **8**) by studying the exploratory behavior of rats in simple mazes. The results of these studies suggest the applicability of this concept to the present results. In this sense, the explanations offered above are considerably more than speculative.

Summary

Two experiments were performed to (*a*) study the effects on spontaneous alternation of varying inter-trial interval and amount of bar-weight simultaneously in a simple **Y**-maze-lever-pressing situation during both acquisition and extinction, and (*b*) determine whether Hull's eighth and ninth postulates provide an adequate basis for explaining alternation behavior. The results show that per cent alternation is related to inter-trial interval and to number of reinforced and non-reinforced trials, but that it is not related to bar-weight.

Although these findings are not incompatible with predictions based upon Hull's eighth postulate, they do not confirm predictions based upon his ninth postulate. It is concluded from this finding and from an analysis of evidence reported by other investigators that Hull's eighth and ninth postulates do not provide a satisfactory explanation of the present results. An alternative explanation is offered: rats tend to alternate R and L choices in a two-choice maze because each arm evokes an "exploratory tendency" that is reduced in strength by the traversal of a given arm but that recovers in strength during a period of non-exposure. This hypothesis, taken in conjunction with the processes of acquisition and extinction, makes possible the explanation of the results of the present experiments as well as those of related studies.

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CONDITIONED DISCRIMINATION AS RELATED TO ANXIETY [1]

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Conditioned discrimination is affected not only by characteristics of the stimuli but also by the temporary sets and predispositions of *S*. Conditioned responses may be modified by information about the stimuli or by instructions regarding stimuli and responses (**3, 4**). It is a reasonable conjecture that responses will be modified also by the more permanent dispositions of *S* commonly referred to as features of his personality. The experiments reported here test the hypothesis that conditioned discrimination will be affected by the relatively persistent apprehensions that make one person more "anxious" than another.

The basic conjecture is that anxious people are likely to see many situations as threatening that other people perceive as neutral and non-threatening. In the conditioned eyelid response experiment, the positive stimulus, reinforced by the air-puff, is a threatening stimulus to both anxious and non-anxious *S*s. The negative stimulus, never followed by the air-puff, is objectively non-threatening, but a person whose discriminations are dimmed by anxiety may not detect the difference between the positive and the negative stimulus. The hypothesis being tested is simply that anxious *S*s, reacting more to their own apprehensions than to carefully discriminated environmental objects and relationships, will exhibit less discrimination of positive and negative stimuli than will non-anxious *S*s.

Because the measure of anxiety adopted is a verbal one (responses to items on a personality inventory) and the measure of conditioned discrimination is relatively non-verbal, the test of the hypothesis must be carried out under the assumption that anxiety as measured by verbal methods is related to anxious behavior in non-verbal situations.[2] Results in agreement with the hypothesis will yield some support for the validity of the assumption of generality of anxiety as tested by verbal methods. It should be noted that dissimilarities of anxiety as measured by the verbal test and anxiety as reflected in behavior in the experimental situation will tend to reduce, under the hypothesis, the degree of relationship between the measured anxiety and conditioned discrimination.

METHOD

Subjects.—The *S*s were all members of a course in introductory psychology, serving in this experiment in fulfillment of part of the laboratory requirement for the course. While a few

[1] This investigation was supported in part by a grant in 1947 to the senior author from the American Philosophical Society. A preliminary account has appeared in the Year Book for 1949 (**2**).

[2] The conditioned discrimination experiment appears at first glance to be both non-verbal and non-social. The *S* reacts to physical stimuli in relative isolation from other human beings. However, there is an *E* present behind a screen and *S* is frequently reminded of his presence by the ready signal given before each presentation of stimuli. The *S* is administered verbal instructions by *E* and attributes to *E* the arrangement of stimuli. There is some tendency for *S* to try to "guess" what *E* is up to. While these aspects of verbal and social behavior inevitably will appear, the situation remains relatively non-verbal in comparison with the personality inventory.

additional *Ss* were used in preliminary work, the final sample consisted of 54 *Ss* for whom both conditioning and anxiety measures were completed.

Of these 54, mechanical difficulties ruined records for four, and four showed no conditioning to the positive stimuli presented in the discrimination series. Since discrimination depends upon some degree of conditioning as evidenced by response to these positive stimuli, the data for these *Ss* were discarded. The anxiety score of the eight *Ss* discarded did not differ significantly from the scores of the 46 remaining *Ss*, so that there is no evidence that dropping them introduced any bias into the results. The 46 *Ss* included 29 men and 17 women.

Conditioned eyelid discrimination.—The unconditioned stimulus consisted of an air-puff delivered to the right eye of *S*. Pressure was kept at 80 mm. of mercury in a system with ample volume so that air pressure was only slightly reduced when the valve was open. The puff continued for 200 msec. and was then cut off. This represents a modification of the air-puff arrangement used in earlier experiments from this laboratory, but provides a better possibility of calibration. In the earlier experiments a low-volume system was used, and the air-puff exhausted the pressure in the system, so that only the initial striking of the eye was at the stated pressure.

The conditioned stimuli, positive and negative, consisted of changes in illumination on adjacent windows of flash glass, each 10 cm. square, separated by 2.5 cm. An illuminated fixation cross appeared between the windows. The windows were 50 cm. from *S*'s eyes, and were viewed binocularly. The steady illumination of the windows was .07 millilamberts and the conditioned stimulus consisted of a sudden increase in illumination to 2.5 millilamberts. The illumination changes were controlled by a rapidly operating silent shutter. The right window served as the locus of the positive CS, the left window as the locus of the negative CS. During reinforcement trials, the illumination of the right window was followed after 650 msec. by the air-puff to the right eye. Movements of the right eyelid were recorded by the shadow of an artificial eyelash of paper attached to the right lid. The stimuli were presented and the responses photographed by means of a Dodge pendulum photochronograph.

During the first conditioning session, the positive CS was presented on each of 60 trials, invariably reinforced by the air-puff. The negative CS was not presented. During the second session devoted to the establishment of discrimination, positive and negative stimuli were presented in a pattern of alternation so arranged that neither would occur more often than twice in succession. There were 30 trials to the positive stimulus, always reinforced, and 30 trials to the negative stimulus, never reinforced.

The two experimental sessions were separated by an interval of from one to four days. For almost all *Ss* the discrimination session followed the first by 48 hr. At each session a set of instructions was read to *S* by *E* who was behind a screen in the room, darkened except for the steady illumination on the windows and for a photographic darkroom light at *E*'s station. Included in the instructions was mention of a ready-signal, the word "Ready" to be spoken by *E* and to be followed by *S*'s fixation on the cross between the stimulus windows. To eliminate the ready-signal as a possible cue for response it was given at varying intervals between 1 and 2.5 sec. before the onset of the CS.

On both days the 60 trials were divided into five sets of 12 and a 2-min. rest period was allowed following each of the first four sets. Approximately 30 sec. separated trials within each set.

In order to avoid including as CR's the β-responses of Grant (1), a CR was defined somewhat arbitrarily as a detectable response (1 mm. or more on the record) beginning not earlier than 250 msec. following the onset of the CS and not later than 650 msec., the time of onset of the air-puff. This limitation of range did not exclude responses that appeared qualitatively to belong to the distribution of CR's. Each record was scored for presence or absence of a CR, unless the record was ambiguous, when it was discarded. Percentage frequencies were based upon scorable records only.

Index of conditioned discrimination.—There is no single satisfactory measure for expressing conditioned discrimination. The first measure to suggest itself is the difference in percentage frequency of response to the positive and negative stimulus. While this does indicate discrimination, the possible difference can never exceed the percentage frequency of response to the positive stimulus. This restriction imposes a relationship between response to the positive stimulus and the discrimination measure which complicates interpretation of results. Perfect discrimination (some degree of response to the positive stimulus and *no* response to the negative stimulus) is indicated by scores varying throughout the range of response to the positive stimulus but limited, for each *S*, by his frequency of response to the positive stimulus.

A second measure considered is a ratio of CR's to the negative stimulus to CR's to the positive stimulus. This has the disadvantages of all ratio scores but has several advantages

over the previously mentioned difference score. The possible range of ratio scores is 0–100 for each *S*, whether or not there has been much conditioning. Failure to discriminate is indicated by a score near 100 while a score near zero shows good discrimination. High scores thus indicate lack of discrimination.

A third measure, actually the simplest, turns out to be in many respects the most satisfactory. This is the percentage frequency of response to the negative stimulus. While this bears a high relationship with response to the positive stimulus (product-moment $r = .73$) the possible range of scores remains independent of the frequency of response to positive stimulus. Further, any *S* who responds at a high frequency to the negative stimulus is not discriminating well. Such a measure has the advantage of being derivable directly from the data, with conversion neither to a difference score nor to a ratio score; interpretation of the score is made easier by this lack of conversion. Again, high scores signify lack of discrimination.

Both the second and the third measures are presented in the analyses which follow. Reliabilities of these measures were estimated by product-moment correlations between the scores on the middle half of trials and scores on the combined first and fourth quarters of trials. When corrected by the Brown-Spearman formula for twice the length of the tests these segment-reliabilities become .73 for the ratio score and .95 for responses to the negative stimulus. The reliability of the ratio score is attenuated by the instability of the measure for those *S*s with low frequency of response to the positive stimulus.

The measure of anxiety.—In preliminary experiments, some efforts were made to derive an anxiety score from Rorschach records, but difficulties in obtaining scores useful for correlational purposes were encountered. Concurrently Taylor (6) had been attempting to relate simple conditioning to anxiety. We were fortunate in obtaining from her the test items she had used to measure anxiety.[3] These were 65 items from the Minnesota Multiphasic Personality Inventory, selected by a jury of five clinical psychologists as likely to measure anxiety. Typical of the items are the following:

I have sometimes felt that difficulties were piling up so high that I could not overcome them.
I have often been frightened in the middle of the night.
I have a great deal of stomach trouble.
I am usually calm and not easily upset.

[3] We wish to express our gratitude to Dr. Taylor and Dr. Spence for permitting us to use the test prepared in connection with the Iowa research project.

While the test as given includes 200 items, only 65 of these are scored. These items yield an anxiety scale with a reliability of .92 as determined by an odd-even correlation coefficient, corrected for length of the test.

The anxiety test was administered to each *S* after he had completed both the conditioning session and the discrimination session. The test was introduced to *S* as a preliminary attempt at selecting items for a personality scale, and he was told that his answers would remain confidential, that the study involved group results and that *E* would not analyze individual scores. The *E*s felt that less "self-conscious" responses might be assured by these attempts to de-personalize the anxiety test situation.

The possible range of anxiety test scores is 0–65, a high score indicative of a high degree of anxiety. The range of scores for the 46 *S*s whose data are presented is 2–31, with a mean of 14.7.[4]

Results

Anxiety scores, simple conditioning and conditioned discrimination.—Our hypothesis does not require a correlation between simple conditioning and anxiety, because the CS is a threatening stimulus to anxious and non-anxious *S*s alike. Conceivably it might be *more* threatening to anxious *S*s, and hence there might be a low positive correlation. The hypothesis does propose, however, that there should be a higher correlation between conditioned discrimination and anxiety, but a negative one. That is, anxious *S*s should show poorer discrimination (more responses to the negative stimulus) than non-anxious *S*s who respond on the basis of the realities of the external situation. The correlations between anxiety, simple conditioning, and two measures of lack of conditioned discrimination, are shown in Table I.

The conjectures, in general, are borne out. The correlation between anxiety and simple conditioning, while

[4] Taylor (6) reports for her *S*s a range of 1–35, with a mean of 14. Thus the two samples appear comparable on this test.

TABLE I

CORRELATIONS BETWEEN ANXIETY, SIMPLE CONDITIONING, AND TWO MEASURES OF LACK OF CONDITIONED DISCRIMINATION ($N = 46$)

	r
(1) Simple conditioning (per cent CR on Day 1)	.12
(2) Lack of discrimination: Ratio of responses to negative stimulus to responses to positive stimulus on Day 2	.37
(3) Lack of discrimination: Responses to negative stimulus on Day 2	.32

positive, is not significant,[5] but the correlations between anxiety and both measures of discrimination approach significance, and are in both cases in the direction conforming to the hypothesis.

Because the coefficients of correlation between the anxiety scores and the measures of discrimination are marginal at best, it seemed desirable to find some other way to express the significance of the relationship. If the data are cast into fourfold tables, as shown in Tables II and III, significant χ^2's result.[6]

[5] This is not to be interpreted as contradictory to Taylor's (6) finding of a relationship between simple conditioning and anxiety. Simple conditioned PGR responses have also been shown to be related to clinically diagnosed anxiety in children by Welch and Kubis (7). Slight differences in experimental arrangements, time intervals, and the like, might easily accentuate or reduce an obtained difference between the responses of anxious and non-anxious *Ss*.

[6] The possibility exists that sex differences in anxiety or conditioning might influence results treated as correlations, or in fourfold tables. Separate plotting of the scores for the 29 male *Ss* and the 17 female *Ss* showed comparable relationships between anxiety and conditioned discrimination for the sexes separately. Sex differences, if any, do not account for the reported correlations. The *Ss* are too few to make an adequate appraisal of sex differences in either anxiety or conditioned discrimination.

Course of discrimination for anxious and non-anxious Ss.—In order to compare the discrimination of anxious and non-anxious *Ss*, two extreme groups were selected as high and low, respectively, on the measure of anxiety. The ten *Ss* with anxiety test scores of 23 or higher are compared with the ten *Ss* with anxiety scores of 7 or below. The mean of the high anxiety group is 25.7; the mean of the low anxiety group is 5.3. Figure 1 shows the results for the means of the two groups, plotted for successive groups

TABLE II

CHI-SQUARE FOR ANXIETY VS. LACK OF DISCRIMINATION: RATIO DISCRIMINATION SCORES

Anxiety Scores	Ratio Scores		
	Low	High	Total
High	6	18	24
Low	17	5	22
Total	23	23	46

$\chi^2 = 12.54 \quad p = .001$[7]

TABLE III

CHI-SQUARE FOR ANXIETY VS. LACK OF DISCRIMINATION: RESPONSES TO NEGATIVE CONDITIONED STIMULUS

Anxiety Scores	Responses to Negative Stimulus		
	Low	High	Total
High	8	16	24
Low	12	8	22
Total	22	24	46

$\chi^2 = 4.22 \quad p = .025$[7]

[7] The p-values are those corresponding to one-half the chance expectancy in typical χ^2 tables. This correction makes the test equivalent to a one-sided χ^2 test, which is appropriate for testing the present hypothesis.

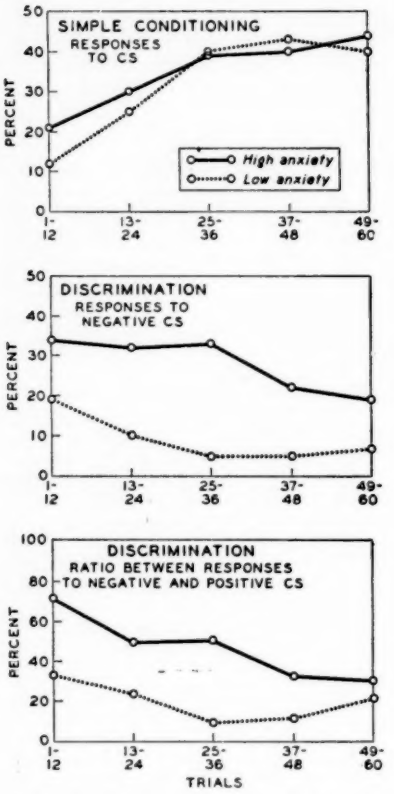

FIG. 1. Course of simple conditioning and discrimination for high and low anxiety groups (10 *Ss* in each group).

of 12 trials, on first-day conditioning, response to negative stimuli during the second (discrimination) session, and the ratio discrimination scores.

It can be seen that the two groups are essentially alike in response to the positive CS on the first day. The groups differ in response to the negative stimulus of the second session, the anxious *Ss* giving the higher frequency of response. Dividing the frequency of response to negative stimuli by the frequency of response to positive stimuli for the second session gives the discrimination ratio. The plot of mean ratios for successive sets of trials once again shows the anxious group to have higher scores (less discrimination) than the non-anxious group.

For all trials combined, the mean differences of the two groups in response to the positive stimulus are not significant on either day. The mean difference in response to the negative stimulus is significant. ($t = 2.91$, $p = .005$). The test of significance of mean differences on discrimination ratio scores gives a t value of 4.57, a corresponding p beyond the .001 level.[8]

CONCLUSIONS AND INTERPRETATIONS

The original conjecture is borne out that more anxious *Ss* would not discriminate as well as less anxious ones. Discrimination depends upon two related but not identical functions: (a) accurately perceiving the events in the environment, and (b) acting appropriate to the probabilities inherent in these perceptions.

It is quite possible that the anxious *Ss* in these experiments knew as well as the non-anxious ones that the left light was never followed by the air-puff. (No reports were obtained from the *Ss*, so that data were not available on this point.) An apprehensive *S*, even though knowing what had happened, might expect *E* to "fool" him later on. It is certainly within *E*'s power to present the air-puff following either stimulus. Hence the apprehensive *S*, with the same perceptual data to go on, might fail to reveal realistic discrimination in his responses, for to him the negative light is still threatening. We are therefore dealing with a complex situation in which habitual apprehension, present

[8] The p-values are those for a one-sided t-test.

set, and accuracy of perceptual discrimination all work together to yield the final behavioral result.

The correlations are not high enough for conditioned discrimination to be offered as a new form of personality test. The results do show, however, that some of the standard laboratory experiments may prove serviceable in the analysis of complex personality factors, of which anxiety is but one. Some of the previous attempts to relate conditioning to personality may have been unsuccessful because of the dependence upon simple conditioning rather than upon conditioned discrimination. Even though a relationship to simple conditioning may occasionally be found, conditioned discrimination is likely to provide a more sensitive measure. This conjecture, earlier stated by Hilgard and Marquis (**5**, p. 303) is supported by the data of the present investigation.

Summary

1. Responses of 46 adult human *Ss* were obtained to a personality inventory designed to measure anxiety, and within a conditioned discrimination experiment. Conditioning consisted in the development of conditioned eyelid responses to an illumination increase on one of two windows, based on the unconditioned eyelid response to an air-puff. Simple conditioning was obtained within one period, followed by discriminatory conditioning in a second period. During the second period, illumination of one window (the positive stimulus) was invariably followed by the air-puff; illumination of an adjacent window (the negative stimulus) was never followed by the puff of air.

2. A positive but non-significant correlation ($r = +.12$) was obtained between anxiety and simple conditioning.

3. Correlations approaching significance were obtained between anxiety scores and two measures of lack of discrimination. The correlation between anxiety and the ratio of negative to positive CR's was .37; that between anxiety and the frequency of negative responses was .32. A χ^2-test confirmed the interpretation that the relationships were significant.

4. The positive relationship between anxiety measures and lack of discrimination supported the conjecture that the investigation was designed to test.

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TWO VARIABLES IN THE ACQUISITION OF DEPRESSANT PROPERTIES BY A STIMULUS [1]

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The presentation of a shock in a situation where escape or termination is not possible will act, at least temporarily, to depress the strength of an ongoing response (**3**). If an S^n (a stimulus which prior to training has relatively little in the way of depressant properties) is repeatedly paired with an unavoidable shock, it tends to acquire depressant properties in that in its presence, an animal's ongoing activity will be reduced. Relevant to this observation, there have been only two studies reported.

Brogden, Lipman, and Culler (**2**), in one of a series of experiments, placed some guinea pigs in a modified activity wheel. For one group of *S*s, a 1000 cycle tone was followed by shock. Since the shock could not be avoided, the animals "literally 'sat tight,' held the breath, and tensely awaited the shock" (**2**, p. 111).

More pertinent to the present problem is the experiment reported by Estes and Skinner (**4**). They showed that a tone which in the past had always been followed by shock acts to depress the rate of bar-pressing. The experiments to be described have attempted to extend the work of Estes and Skinner and to determine the degree to which an S^n will acquire depressant properties if paired with a stimulus like shock. The two variables under consideration were the duration of interval between the onset of the continuous S^n and the shock, and the number of pairings between the continuous S^n and the shock. As in the Estes and Skinner experiment, the strength of the depressant properties acquired by the S^n was measured by its effect upon an already well-established reflex.

METHOD

Apparatus.—The apparatus made it possible to work with ten animals simultaneously. The ten experimental cages and the living cages were the type described by Frick, Schoenfeld, and Keller (**5**). The bars were slightly modified, being constructed of stainless steel tubing of $\frac{3}{8}$ in. outer diameter to prevent biting. Each experimental cage was equipped with an automatic feeding device. When the automatic feeder was connected to the bar, each pressing of the bar released a pellet of food down a chute to the food cup in the cage. When the food magazines were not filled, each pressing was still accompanied by the usual apparatus click, but no pellet was delivered. Bar pressings were recorded upon a constant-speed wax-tape polygraph (**5**), which made it possible to obtain response rates as well as total number of responses.

The S^n was the illumination from a 10 w., 110 v. bulb covered with three layers of frosted paper and placed directly above each of the ten experimental cages. The ten bulbs were wired in parallel and their onset and termination were controlled by one master switch. The illumination of the cage when the light was on was .88 foot-candles, and .04 foot-candles without the light.

The shock consisted of 500 v. passing through a 250,000 ohm resistance in series, and was delivered through ten floor grids wired in parallel. The resistance of each rat standing on the grid was approximately 1 megohm. The grids consisted of brass strips, $\frac{1}{4}$ in. wide, spaced $\frac{3}{4}$ in.

[1] A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Faculty of Pure Science, Columbia University. The writer wishes to express his profound gratitude to Professor W. N. Schoenfeld, under whose guidance this research was performed. The writer also wishes to express special thanks to Dr. P. J. Bersh, without whose constant encouragement and valuable suggestions this work would not been have completed.

[2] Now at Indiana University.

apart and mounted on a wooden frame. One switch affected the simultaneous delivery of the shock to the ten experimental cages. On the days when *Ss* were shocked, panels consisting of ¾ in. plywood blocked the front and rear ends of the cages completely so that *Ss* might not escape the shock by climbing on the food trays and water bottles.

The experimental and living cages remained in the same room throughout the experiment, and *Ss* were carried by hand from the living cages to the experimental cages. Screens were placed in front of the living cages on the days the light, the S^a, was to be paired with the shock. The experimental room itself was lighted at all times by low ceiling illumination and noise was kept at a low level.

Experiment I.—The first experiment attempted to answer the questions: To what extent does an S^a acquire depressant properties if the interval between the onset of a continuous S^a and a shock is varied. Seventy male rats of Wistar stock, approximately 120 days old at the start of the experiment, were divided at random into seven groups of ten animals each. A whole group of animals was run during each 45-min. experimental session. The seven intervals between the onset of the light and the shock were 0, 1, 4, 7, 10, 20, and 30 sec. The drive condition throughout the whole experiment was 22 hr. food deprivation.

The *Ss* were first placed on a feeding rhythm for nine days. During the next three days, each group received ten pairings of light and shock on the value of light-shock interval assigned to that group. The paired stimuli were presented at irregular intervals ranging from 2 to 10 min. and no escape or avoidance was possible. The shock was of 1-sec. duration in the presence of the light, and light and shock were terminated at the same instant. The light, therefore, was on for 1 sec. more than the interval defined by its onset and the onset of the shock.

Following the shock days, the *Ss* were trained in bar-pressing under regular reinforcement for three days. The reinforcing pellets of food were made of Purina lab chow, the same food as the normal diet, and the mean weight of the pellets was 1/20 gm. Since the *Ss* were capable of eating 100 to 300 pellets during the experimental session, no *S* received less than 300 for the three days, and this probably sufficed to bring the response of bar-pressing to maximum strength (13).

The fourth day of conditioning was the first test day. The *Ss* were given in sequence: (a) 10 min. of regular reinforcement in the dark, (b) 10 min. of regular reinforcement in the continuous presence of the lights, and (c) 25 min. of regular reinforcement in the dark. All bar-pressings were reinforced whether they occurred during the light or the dark periods.

The following test days were extinction days for the bar-pressing response, i.e., no reinforcements were given. The extinction schedule included the same sequence as the reinforcement schedule. Test days were continued through the second extinction day, at which time there were no differences among the groups.

Experiment II.—In the second experiment, the independent variable was the number of combinations of light and shock. Sixty male rats of Wistar stock, approximately 120 days old, were divided at random into six groups of ten animals each. The animals were first placed on a feeding rhythm for nine days, and food deprivation of 22 hr. was the drive condition throughout the experiment. The groups were given 0, 5, 10, 20, 40, and 80 pairings of light and shock, with a 7-sec. duration of continuous light before the onset of the shock, after which both light and shock lasted one more second. The shock schedule in this experiment was spread over eight days in order not to allow more than ten pairings of light and shock per day. The 80-pairing group was given 10 light-shock combinations a day, the 40-pairing group 5 a day, the 20-pairing group 2 for the first four days and 3 for the last four days, the 10-pairing group 1 for the first six days and 2 for the last two days, and the 5-pairing group 1 for the last five days.

Following the light-shock training, *Ss* were conditioned on bar-pressing to allow this response to reach high or near maximum strength. Test days followed the three days of conditioning. Since the first experiment showed that one extinction day of the bar-pressing response was enough to nullify any differences among the groups, in this experiment the first three test days were made under conditions of regular reinforcement of the bar-pressing response. The same test sequence held, however, as in Exp. I.

Since the control group, or the 0-pairing group, was run with groups of the first experiment, it could only be compared with the other five groups of the second experiment on the first test day.

Treatment of results.—The measure of depression was the difference in rate of bar-pressing between the initial 10-min. dark period and the 10-min. light period on the test days. Two measures were taken for each test day. The first 5-min. in the dark were compared with the first 5-min. in the presence of the light, and the full 10-min. in the dark before the onset of the light were compared with the full 10-min. in the continuous presence of the light.

Since the temperature of the experimental room varied from 78°–96° F., and since this wide fluctuation of temperature affected response rate,

it seemed necessary to equate the groups for initial rate of bar-pressing on the test days before the introduction of the test stimulus. The drops in rate of responding were corrected by means of covariance. The adjustments of the drops in rate of responding were based on regression coefficients derived from the within-groups sum of squares for pre-drop response totals and the within groups cross-products of pre-drop values with unadjusted drop values (**8**, **9**). This procedure leveled out the differences in the initial rates of responding before the introduction of the test stimulus, making the depression a reflection of the experimental procedures alone. Corrections were made for the 5- and 10-min. comparisons separately, and for each test day separately.

In addition, the groups were compared in the number of responses made in the 5-, 10-, and 25-min. periods following the termination of the light. These response totals were also corrected by covariance on the basis of the pre-drop totals.

Results

Experiment I.—The results for the first test day when all responding was being regularly reinforced are shown in Table I. The corrected mean drops for the 5-min. and 10-min. test periods are graphed in Fig. 1. When tests of significance were applied to the data of the 5-min. test period, it was found that the 0-sec. group differed significantly (.01 level) from the 1-sec. group, the 1-sec. group differed significantly (.05 level) from the 7-sec. group, and the 4-sec. group differed significantly (.02 level) from the 7 sec. group. In the 10-min. test period, the 0-sec. group differed significantly (.02 level) from the 1-sec. group, the 4-sec. group differed significantly (.01 level) from the 7-sec. group, and the 20-sec. group almost reached the .05 level of significance from the 30-sec. group.

The amount of depression for each group was not constant throughout the 10-min. test period, but rather each group recovered to some extent, though differentially. In Fig. 2 are shown the uncorrected minute by minute drops for three groups, the control group which is the 0-pairing group of the second experiment, the 1-sec. interval group, and the 10-sec. interval group. For each of these three groups, the first minute in the dark was compared with the first minute in the light, etc. These three groups were chosen for comparison because their initial rates in the dark were similar, and hence no covariance corrections were needed. The control group had no history of light-shock combinations and was introduced to the light on the first test day.

TABLE I

Comparison of Frequencies of Bar-Pressing Response during Dark and Light Periods on First Test Day as a Function of Duration of Light during Training

(N = 10 each condition)

Duration of Light in Sec.	Five Minute Test						Ten Minute Test					
	Dark		Light		Diff.	Corrected Diff.	Dark		Light		Diff.	Corrected Diff.
	M	*SD*	*M*	*SD*			*M*	*SD*	*M*	*SD*		
0	63.4	16.1	29.5	16.2	33.9	22.9	112.2	31.7	69.9	25.7	42.3	23.8
1	52.0	14.6	12.1	11.9	39.9	39.3	94.2	21.5	45.1	21.1	49.1	47.8
4	46.6	15.1	15.5	13.8	31.1	35.5	87.1	28.3	37.8	25.3	49.3	55.2
7	45.0	13.5	2.8	6.8	42.2	48.1	82.1	18.9	11.0	19.7	71.0	81.9
10	52.2	12.8	4.8	6.2	47.4	46.7	92.5	16.5	17.6	19.0	74.9	75.3
20	46.0	12.4	3.1	5.2	42.9	47.9	81.0	13.1	8.4	15.7	72.6	84.6
30	54.4	21.6	6.9	6.8	47.5	44.7	101.1	32.0	25.6	20.4	75.5	67.2

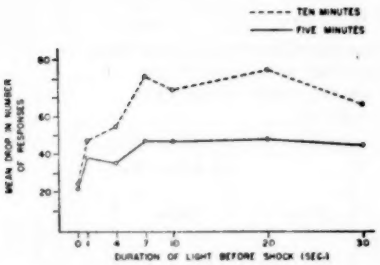

FIG. 1. Corrected mean drops in number of responses made in the 5- and 10-min. test periods on the first test day. Each duration of light represents one group of ten *S*s.

In Fig. 2 it can be seen that the 10-sec. group, for which the light was most effective, recovered least by the end of the light period. The light, when first presented to the control group, exerted as great a depressive effect upon the bar-pressing response as for the other groups, showing that the light was not a "neutral" stimulus. The control group, however, recovered completely. The below zero results of the control group indicate that the members of this group were responding at a faster rate during the latter minutes of the light period than during the latter minutes of the first 10-min. dark period. This type of "compensation" in rate perhaps might have occurred in the 1-sec. and 10-sec. groups if the light period were longer than ten minutes. The curves still seem to be dropping at the end of the 10-min. light period.

When the light was terminated, the groups continued to respond at a differential rate. Those groups for which the light was most effective as a depressant continued to respond on the whole at a slower rate after the light was turned off, but the trend is not as systematic as in the case of the drop.[3]

[3] For detailed material supplementary to this article order Document 2926 from American Documentation Institute, 1719 N Street, N.W., Washington 6, D. C., remitting $0.50 for microfilm (images 1 in. high on standard 35 mm. motion picture film) or $1.00 for photocopies (6 × 8 in.) readable without optical aid.

The results for the second test day, an extinction day for the bar-pressing response, were similar to those of the first test day, but the drop between the 20-sec. and 30-sec. groups during the 10-min. test period was not present. After the termination of the light, there were no differences in the rate of responding between the groups. It would seem that those groups for which the light was most effective recovered the most from its effect to equal the response rates of the other groups. No differential results appeared on the third test day, which was the second extinction day for the bar-pressing response.

Experiment II.—The results of the first test day under conditions of regular reinforcement after various numbers of light-shock pairings during training are presented in Table II. The drops in rate of responding corrected by covariance for pre-light differences in response levels are shown in Fig. 3. Tests of significance applied to the data of the 5-min. test period showed that the 0-pairing

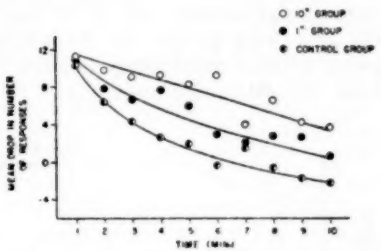

FIG. 2. Minute by minute drop in number of responses for three groups on the first test day. Each of the first ten minutes in the dark is compared with the corresponding minute of the second ten minutes in the light.

TABLE II

COMPARISON OF FREQUENCIES OF BAR-PRESSING RESPONSE DURING DARK AND LIGHT PERIODS ON FIRST TEST DAY AS A FUNCTION OF FREQUENCY OF LIGHT-SHOCK PAIRINGS DURING TRAINING
(N = 10 each condition)

No. of Light-shock Pairings	Five Minute Test						Ten Minute Test					
	Dark		Light		Diff.	Corrected Diff.	Dark		Light		Diff.	Corrected Diff.
	M	*SD*	*M*	*SD*			*M*	*SD*	*M*	*SD*		
0	53.7	22.1	28.3	11.9	25.4	23.4	97.3	28.8	76.0	26.5	21.3	15.9
5	46.7	7.8	18.0	9.7	28.7	30.2	90.5	10.4	55.1	11.7	35.4	35.6
10	44.1	13.0	10.1	11.8	34.0	36.7	81.4	23.7	24.1	24.7	57.3	64.9
20	52.1	8.2	9.8	12.9	42.3	41.1	94.5	15.4	26.6	27.6	67.9	64.8
40	49.1	27.0	4.0	5.0	45.1	45.4	87.7	13.9	16.6	17.7	71.1	73.6
80	52.2	9.2	18.4	13.2	33.8	32.6	92.5	15.1	46.4	23.8	46.1	44.6

group differed significantly (.05 level) from the 10-pairing group, the 0-pairing group differed significantly (.01 level) from the 20-pairing group, the 5-pairing group differed significantly (.02 level) from the 40-pairing group, and the 20-pairing group differed significantly (.05 level) from the 80-pairing group. In the 10-min. test period, the 0-pairing group differed significantly (.05 level) from the 5-pairing group, the 5-pairing group differed significantly (.01 level) from the 10-pairing group, the 40-pairing group differed significantly (.01 level) from the 80-pairing group, and the 20-pairing group differed significantly (.05 level) from the 80-pairing group.

Uncorrected minute by minute drops in rate of responding in the presence of the light for three groups are shown in Fig. 4. The three groups chosen were the 0-pairing group, the 40-pairing group, and the 80-pairing group, since these groups had similar response rates during the initial 10 min. in the dark and covariance corrections were not necessary. From Fig. 4 it can be seen that the 40-pairing group, for which the light was most effective, recovered the least in the presence of the light. The control

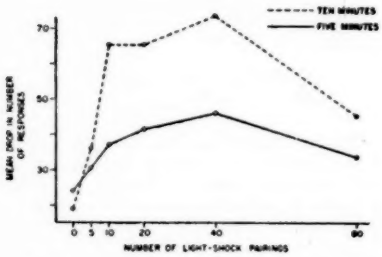

FIG. 3. Corrected mean drops in number of responses made in the 5- and 10-min. test periods on the first day. Each number of pairings represents one group of ten *Ss*.

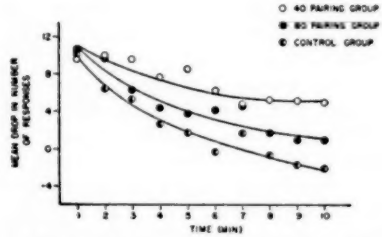

FIG. 4. Minute by minute drop in number of responses for three groups on the first test day. Each of the first ten minutes in the dark is compared with the corresponding minute of the second ten minutes in the light.

group, for which the light was least effective, recovered the most. It is possible, as in Exp. I, that the "compensation" exhibited by the control group might have been shown by the other groups if the light period had been extended. In contrast with the first experiment, no differences in the rate of responding followed termination of the light. There was no evidence that those groups for whom the light was most effective responded at a slower rate when the light was turned off.

The results of the second and third test day under conditions of regular reinforcement were similar to those of the first test day except that the maximum shifted from 40 to 20 pairings. No differential results appeared on the fourth test day, which was the first extinction day for the bar-pressing response.

Discussion

The results of the first experiment seem to indicate that as the interval between the onset of a continuous S^a and shock was lengthened from 0 to 10 sec., the light acquired greater depressant properties in terms of the response that was measured. The light did not seem to acquire any greater depressant effect for the intervals tested beyond 10 sec. Furthermore, the 10-min. results of the first test day suggest that, beyond a certain interval of about 20 sec., the light began to lose its effectiveness as a depressant, i.e., the function has a maximum. Another group of animals, whose performance is not reported here because of apparatus failure during their training, was run through the experiment with a duration of light of 60 sec. before the onset of the shock. The results for this group suggested that the effectiveness of the light dropped considerably for intervals longer than 30 sec., but this remains to be corroborated.

Although the suggested presence and position of a maximum in the present experiment seems inconsistent with studies of stimulus-response asynchronism, most of these studies employed an operant avoidance response to measure the effective interval between the conditioned and unconditioned stimulus (**1, 7, 10, 12, 14, 15**) and not a "pure" respondent as defined by Skinner (**11**). Kappauf and Schlosberg (**6**) measured a respiratory response, but this represents an overlap of operant and respondent reflexes. In the case of glandular secretions, such as salivation in dogs, no experiment has been reported where the interval between conditioned and unconditioned stimulus was varied systematically. In neither the glandular nor the non-glandular studies was the *time* of maximum amplitude of the conditioned response reported. In the absence of further information concerning the conditioning of glandular responses, an explanation of the results of the first experiment would seem premature at this time.

The tentative conclusion to be reached from the second experiment is that as the number of light-shock combinations increased, the depressant effect of the light seems to go through a maximum. The writer has been unable to find any case reported where additional presentations of an operationally unchanged reinforcement results in a loss in strength of a conditioned response. There is a possible explanation for the present results which can be tested experimentally. Zeaman (**16**), using a runway technique, reduced the amount of reinforcement after latency asymptotes had been reached. He found that this reduction resulted in a latency increase and interpreted this as

a partial extinction phenomenon. If Zeaman's interpretation of a reduction in reinforcement as an extinction phenomenon is correct, then this might have occurred in one of two ways in the second experiment.

The first is that some desensitization or "adaptation" process occurred which reduced the effectiveness of the shock on the subsequent light-shock trials. For many receptors, such as olfaction and pain receptors, continued presentation of a stimulus results in a reduction of its effect, especially if the stimulus is not particularly strong. The amount of reinforcement might also have been reduced through partial avoidance of the shock stimulus. It was noticed in the second experiment, in the latter light-shock trials for the 80-pairing group, that the animals seemed more "agitated" than the other groups had been. The 80-pairing group may have had more opportunity to learn to reduce the effect of the shock completely. These two possibilities can be tested experimentally.

Summary

1. It is known that an S^a which precedes a stimulus-like shock will acquire response-depressant properties. In Experiment I, seven groups of rats were given 30 pairings of a continuous light and a shock with intervals of 0, 1, 4, 7, 10, 20, or 30 sec. between the onset of these stimuli. The depressant effect of the light was measured on an already well-established bar-pressing response, both while the response was near maximum strength and still being reinforced, and while the response was being extinguished.

2. The function showed a rapid rise in effectiveness of the light to the -7 to 10-sec. intervals. No great loss in effectiveness was observed beyond this point for the intervals tested. The 10-min. test results of the first test day seemed to suggest, however, that beyond the 20-sec. interval, the light begins to lose its effectiveness as a depressant, i.e., the function has a maximum. On the first test day, those groups for which the light had the most depressant effect continued to respond at a slower rate after the termination of the light. The results of the second test day (extinction) were similar to those of the first with the exception that the drop between the 20-sec. and 30-sec. groups during the 10-min. test period was absent. No differential results were obtained on the third test day, i.e., the second day of extinction for the bar-pressing response.

3. In Experiment II, six groups of rats were given 0, 5, 10, 20, 40, and 80 pairings of light and shock. The interval between the onset of the continuous light and the shock was held constant at 7 sec. The depressant effect of the light was tested as in Experiment I.

4. The first three test days were made under conditions of regular reinforcement for the bar-pressing response. The depressant effect is at a maximum at 40 pairings on the first test day, and at 20 pairings on the second and third test days. The fourth test day, when responses were not being reinforced, showed no differences among the groups. The groups did not show any differential response rates after the termination of the light on any test day.

5. An explanation for the presence of the maximum is discussed in terms of the possible reduction in the amount of reinforcement. This reduction in the amount of shock might have occurred through (a) a possible "adap-

tation" effect of shock, and (b) partial avoidance of the shock stimulus.

(Manuscript received September 1, 1950)

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DRIVE (D) AS A FUNCTION OF HOURS OF HUNGER (h) [1]

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A considerable amount of experimental investigation has been conducted in various drive modalities to determine the effect of intensity of motivation on various manifestations of learning. These studies have resulted in the establishment of relationships of quantifiable manifestations of behavioral potentiality to the experimentally varied units of deprivation. However, these units are in terms of hours of deprivation, or some other external condition, and not in terms of the internal drive condition. For example, in the studies by Warden *et al.* (**11**) changes in obstruction grid crossings were produced by varying the hours of food, sex, or water deprivation. By a reconditioning procedure, Heron and Skinner (**3**) determined the relationship of rate of unreinforced responses to days of hunger. Also in the experiments by Williams (**15**) and Perin (**9**), recently expanded by Koch and Daniel (**8**) and Saltzman and Koch (**10**), intensity of motivation was varied by varying the number of hours of food deprivation, and approximations to measures of reaction potentiality were obtained by extinguishing the conditioned Skinner box lever-pressing response.

With the reaction potential quantification methodology recently elaborated by Hull *et al.* (**6**) it is now presumably possible to state drive as a quantified construct, and to arrive at a determination of the quantitative law of the functional relationship of drive intensity as a quantitative construct to such a directly quantifiable variable as hours of food deprivation. In the Hullian system of interrelated constructs it is postulated that behavioral reaction potential (${}_sE_R$) is basically determined by two mediating internal conditions: habit strength (${}_sH_R$) and drive (D). With suitable quantification, it is believed that a multiplicative relationship exists between habit strength and drive such that,

$$ {}_sE_R = f({}_sH_R \times D). \qquad \text{(a)} $$

Thus, if the deprivation level is kept constant during the course of a learning process, the increases in reaction potential on successive reinforcements will be essentially a function of the increase in habit strength. On the other hand, with the conditions of reinforcement (**5**, p. 178) held constant between several groups in a given experimental situation, the differences in reaction potential will be produced by differences in drive. In making comparisons between these several groups when habit strength (${}_sH_R$) is constant, equation (a) will reduce to,

$$ {}_sE_R = f(D). \qquad \text{(b)} $$

In a previously reported study (**17**) the quantitative relationship of reaction potential (${}_sE_R$) to number of extinction reactions (n) was determined in an experimental situation where drive was held constant and the

[1] The present study is a portion of a thesis presented to the Faculty of the Graduate School of Yale University in partial fulfillment of the requirements for the degree of Doctor of Philosophy. The investigation also represents a part of the coordinated research program of the Institute of Human Relations. The author is deeply indebted to Professors Clark L. Hull and Mark A. May for their generous advice and helpful criticism.

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habit strength associated with a Skinner box manipulandum response was varied by giving several groups of albino rats different numbers of reinforcements before extinction. Knowing the relationship ${}_sE_R = f(n)$ and $n = f(h)$, where h is hours of hunger, under the conditions of equation (b) we are in position to secure

$$D = f(h). \qquad (c)$$

Even if D were multiplied by a constant of less than 1.0 in the step represented by equation (b), the general nature of the law of $D = f(h)$ could be determined.

A schematic representation of the supposed functional relationships involved in this determination is as follows:

$$\left.\begin{array}{l}\text{(I) } {}_sE_R = f(N)\\ \text{(II) } n = f(N)\end{array}\right\}\text{(III) } {}_sE_R = f(n)\left.\begin{array}{l}\\ \text{(IV) } n = f(h)\end{array}\right\}\left.\begin{array}{l}\text{(V) } {}_sE_R = f(h)\\ \text{(VI) } {}_sE_R = f(D)\end{array}\right\}\text{(VII) } D = f(h),$$

where, ${}_sE_R$ = reaction potential, N = number of reinforcements, n = number of extinction reactions, h = hours of hunger, and D = drive.

In the study referred to above (**17**), the first three of the preceding functional relationships, with ${}_sE_R$ in terms of superthreshold reaction potential (${}_sE_R$), were found to be:

$$\text{(I)} \quad {}_sE_R = 7.58\,[1 - (N+1)^{-.201}] + .859;$$

$$\text{(II)} \quad n = 23.00\,(1 - 1.306\,N^{-.749}) - .11;$$

$$\text{(III)} \quad {}_sE_R = .1225 \times 10^{.0847\,n} + 2.114.$$

If the above series of functional equations I to VII were perfect, it would be possible to deduce III from I and II, V from III and IV, and VII from V and VI. Unfortunately, as we have found previously (**2**, p. 525), in the present embryonic state of behavior science these equations are not sufficiently accurate to serve such exact usage. It is necessary, therefore, to proceed by the method of substituting empirical data and to fit equations to the emerging values.

The purpose of the present study, then, is to utilize this method to quantify the drive construct as a function of the directly measurable external variable of hours of food deprivation, other factors constant, i.e.,

$$D = f(h).$$

Method

Subjects.—The 219 male albino rats used in this experiment were similar in age, weight, and source to those of the first study of this series (**17**). They ranged from 7 to 8 months in age at the beginning of training, with a mean satiated weight of 248.1 gm. The percentage of the 290 original animals lost through starvation or incapacitating injuries was 24.5 per cent. All aspects of the feeding, satiation, and housing procedures were identical with those of the previous study. The *S*s were randomly assigned to the various experimental groups listed in Table I. Groups C_1 and C_2 corresponded to groups Va and Vb respectively of the first study (**17**).

Apparatus.—The apparatus was the modified Skinner box described previously (**17**). Essentially it consisted of a sound-shielded box enclosing an animal compartment. A shutter controlled *S*'s access to the manipulandum and also provided a means of obtaining response latencies. The learned response was a leftward movement of the horizontally protruding manipulandum. Specially prepared pellets averaging approximately .2 gm. with a 50 per cent water content by weight were used as reinforcement.

Procedure.—A group of *S*s was trained and extinguished on the manipulandum response at each of the following hunger intervals: 3, 12, 24, 48, and 72 hr. Maintenance, habituation, training, and extinction procedures were identical to those used in the first study.

The maintenance procedure used for *all* hunger groups consisted of a 5-day cycle of 72

hr. of hunger followed by 48 hr. of feeding *ad libitum* and ending with enforced satiation. Each group was given experimental treatment at the appropriate time within the 72-hr. hunger period. This procedure of maintaining all groups on the same feeding cycle resulted in a fairly equal proportionate loss of subjects among the groups. The differences in these proportions are not significant since the chi-square is only 2.65 with 5 *df*; a chi-square as large or larger could occur 75 per cent of the time by chance.

Experimental trials for all groups were conducted from mid-morning to early afternoon. It was, therefore, necessary to feed and satiate the 12-hr. group at night and the 3-hr. group in the morning.

During habituation and training, trials were given on a quasi-distributed schedule of four trials per session, one session per five days. Since only one training session was given every five days, each hunger group was subdivided into five subgroups so that a portion of each group could be given experimental treatment each day.

On the habituation trials the rats were trained to approach and seize a pellet protruding from the pellet-holding tube after the shutter was raised, the pellet-dispensing mechanism being operated by *E*. Each group was given habituation training at the hunger level to be used for training and extinction. Habituation was continued until all the groups were equal with respect to the latency of the habituation response. This point of equality was reached after 28 pellet presentations; the mean of the medians of the last four trials for individual *Ss* was 2 sec. for all groups.

On the session following the last habituation session, the manipulandum was introduced for the first time and training trials were begun.[3] All *Ss* except those of Group C_2 were given a test trial at each of three nontraining hunger levels. These trials were introduced to obtain data for a study on drive stimulus generalization and were distributed between training trials 72–73, 76–77, and 80–81.

After 88 training trials, massed extinction trials were given, *each group being extinguished at its training level of hunger*. The previously used extinction criterion of 2 min. was applied.

TABLE I

NUMBER OF *Ss* IN GROUPS WHICH VARIED IN THE NUMBER OF HOURS OF HUNGER (*h*) DURING TRAINING AND EXTINCTION

Group	Hours of Hunger (*h*)	Number of *Ss*	Original Number of *Ss*
A	3	42	51
B	12	38	51
C_1*	24	38	51
C_2*	24	28	35
D	48	36	51
E	72	37	51

* Groups C_1 and C_2 are Groups Va and Vb in the previous study (17).

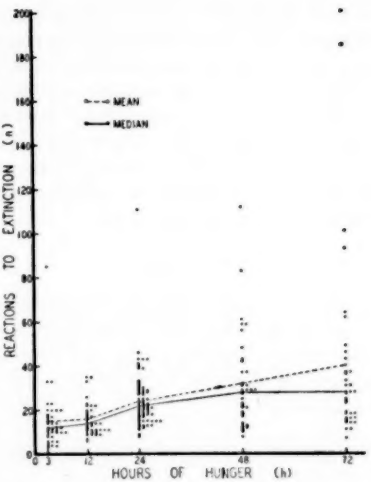

FIG. 1. The distribution of the number of extinction responses of the various hunger groups. Each dot represents an individual animal. The data of the two 24-hr. subgroups have been pooled.

RESULTS

Trials to Extinction (n) as a Function of Hours of Hunger (h)

The experimental results are shown in Fig. 1, in the form of dot distributions for the various hunger groups.

[3] In the 3-hr. group, eight *Ss* failed to respond within 15 min. on the first trial. The manipulandum-baiting technique (1) was then used on these *Ss*. In all cases, one reinforced response to the baited manipulandum was sufficient to establish the response to the unbaited manipulandum. The first-trial response latency was considered to be 15 min. for these animals.

TABLE II

EFFECT OF HOURS OF HUNGER (h) DURING TRAINING AND EXTINCTION ON THE NUMBER OF REACTIONS TO EXTINCTION (n)

Group	Hours of Hunger	*Ss*	No. of Responses to Extinction		
			Mean	Median	$\sigma_{Mdn.}$
A	3	42	14.90	12.00	2.49
B	12	38	15.76	14.00	1.47
C	24	66	24.45	22.00	2.17
D	48	36	32.25	28.50	4.52
E	72	37	39.97	28.00	8.66

Each dot represents the number of trials to extinction for an individual *S*. The data of groups C_1 and C_2 at 24 hr. have been pooled, since their homogeneity was established in the previous study (**17**). The positive skew of the distributions is readily apparent, as are the greater values of the means over the medians. Although the means continue to rise throughout the tested range of hunger, the medians show a decline after 48 hr. The mean and median number of trials to extinction shown in Fig. 1 are presented in Table II; the differences between the medians and significance of these differences appear in Table III.

In the previous study (**17**) the relationship of superthreshold reaction potential ($_sE_R$) to number of extinction reactions (n) was determined to be,

$$_sE_R = .1225 \times 10^{.0647\,n} + 2.114. \qquad (1)$$

Furthermore, superthreshold reaction potential was determined to be 1.155 σ less than reaction potential based on absolute zero ($_s\dot{E}_R$), so that,

$$_s\dot{E}_R = {}_sE_R + 1.155. \qquad (2)$$

Therefore, the relationship $_s\dot{E}_R = (fn)$ is,

$$_s\dot{E}_R = .1225 \times 10^{.0647\,n} + 3.269. \qquad (3)$$

TABLE III

SIGNIFICANCE OF DIFFERENCES IN MEDIAN NUMBER OF REACTIONS TO EXTINCTION BETWEEN PAIRED GROUPS

Paired Groups	Difference	Critical Ratio	*P*
3 hr. vs. 12	2.0	0.690	.49
3 hr. vs. 24	10.0	3.027	<.01
3 hr. vs. 48	16.5	3.195	<.01
3 hr. vs. 72	16.0	1.776	.07
12 hr. vs. 24	8.0	3.052	<.01
12 hr. vs. 48	14.5	3.048	<.01
12 hr. vs. 72	14.0	1.594	.11
24 hr. vs. 48	6.5	1.296	.20
24 hr. vs. 72	6.0	0.672	.50
48 hr. vs. 72	0.5	0.051	.96

The reaction potentiality associated with the various hunger groups was calculated by substituting the empirically obtained n-values in equation (3). These calculated $_s\dot{E}_R$ values are shown in the third column of Table IV.

By pairing the various hours of hunger and the $_s\dot{E}_R$ values associated with each value of n, the relationship of absolute reaction potential to hours of hunger can be shown. This relationship is graphically presented in

TABLE IV

MEDIAN NUMBER OF REACTIONS TO EXTINCTION (n) WITH THE CORRESPONDING ABSOLUTE REACTION POTENTIAL ($_s\dot{E}_R$) VALUES FOR VARIOUS HOURS OF HUNGER (h)

Hr. of hunger (h)	Median No. of Reactions to Extinction (n)	Absolute Reaction Potential ($_s\dot{E}_R$) as Calculated*	Fitted $_s\dot{E}_R$ Values**
3	12.0	4.001	4.000
12	14.0	4.255	4.173
24	22.0	6.516	6.518
48	28.5	11.821	11.895
72	28.0	11.207	11.207

* Calculated from equation (3).
** Obtained by fitting the curve shown in Fig. 2 to the data of columns 1 and 3 and by equation (4).

Fig. 2. The least squares curve fitting the data shown in this figure is based on the equation,

$$_{S}\dot{E}_{R} = [37.824 \times 10^{-27.496(1/h)} + 4.001][1 - .00001041\ h^{2.486}]. \quad (4)$$

This equation has a mean square deviation of .0024 σ^2.

The concavity in the curve between three and approximately 24 hr. is similar to the finding of Perin (**9**), who trained albino rats to a Skinner box lever-pressing response at 23 hr. of hunger and extinguished this response at various lower hunger levels. Holden (**4**) also found similar results in a shock-obstruction experiment in this deprivation range for the median number of crossings as a function of hours of hunger. For two of the three intensities of shock used, a concavity appeared in the curve between zero and 24 hr. of hunger. This point of comparison cannot be made with the experiments by Warner (**13**) and Heron and Skinner (**3**), since no tests were made between zero and 24 hr.

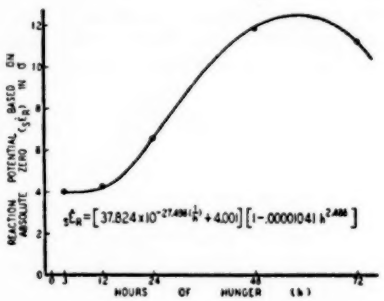

Fig. 2. Graphic representation of the relationship of absolute reaction potential ($_{S}\dot{E}_{R}$) to hours of hunger (h). The absolute reaction potential values are based on reactions to extinction. Each group was extinguished after 88 reinforcements. Note that the curve does not extend to hunger values below 3 hr. The results of other studies (**8, 10**) suggest that the curve falls sharply from approximately two to zero hours of food privation.

It can also be seen from the curve in Fig. 2 that the empirical maximum in reaction potential based on median extinction responses is at 48 hr., and that the theoretical maximum is between 48 and 72 hr. Further increase in the hours of hunger produces a decrease in reaction potential. This effect was found in all the studies just cited in which the hours of hunger were extended beyond 24, but the point in hours of hunger at which the maximum occurs differs considerably. For the Holden animals given the degree of shock most similar to that used by Warner, the maximum number of grid crossings occurred at 36 hr. The maximum number of reactions to extinction in the present study was obtained at 48 hr. In the study by Warner, the maximum number of grid crossings was made at 96 hr. In the Heron-Skinner reconditioning experiment the maximum number of unreinforced responses per hour was produced at 120 hr. The various differences of procedure obtaining between these experiments indicate that the two main factors associated with differences in the point in hunger when the maximum reaction potentiality occurs are (1) degree of inanition and (2) method of measuring response strength.

The first of these two factors was undoubtedly operating most strongly in Holden's rats, which showed the earliest maximum, since they were only approximately 8-weeks-old and ranged in weight from 80 to 110 gm. The stronger and earlier effect of hunger-produced enfeeblement on these animals is apparent in the high proportion of starvation deaths: 14.2 per cent of all *S*s in the 60-hr. group and 40.6 per cent of the 72-hr. group died. Although the rats of the present study were comparable in age to those of the Warner and Heron-Skinner experiments, 24.5 per cent of the original number of *S*s were lost through starvation, whereas no losses occurred earlier than 72 hr. in the latter two studies. It will be recalled that in the present study all hunger groups were kept on a maintenance cycle

of 72 hr. of hunger followed by 48 hr. of feeding, with a number of repetitions of this cycle occurring during the course of habituation and training. This procedure differed from that of the other studies, where the animals were exposed only once to their experimental food-privation period. Since repeated exposure to 72 hr. of hunger caused more deaths in our mature rats, the surviving animals on whom all of the data are based were undoubtedly more enfeebled than those of the two other studies where the maximum was reached later.

The second factor which concerns the differences in procedure and method of measuring reaction potentiality is more difficult to demonstrate, since the four experiments being considered here differ from each other in more than one respect. From the data of Warner's study (**13**, p. 294), however, it is evident that if a 10-min. test period had been used as in the Holden experiment the maximum number of grid crossings for males and females combined would have occurred at 48 instead of 72 hr. Since the Heron-Skinner experiment involved nonreinforced responses in a reconditioning procedure instead of obstruction grid crossings, a direct comparison cannot be made. It is to be noted, however, that 1-hr. test periods were used in the Heron-Skinner study. The general implication would seem to be that when persistence of response is measured, either by the obstruction or the nonreinforcement method, the longer the test period, other things equal, the later the maximum will occur.

It will be observed that the curve in Fig. 2 is not extended below 3 hr., the lowest h value tested. The studies by Koch and Daniel (**8**) and Saltzman and Koch (**10**) at hunger intervals between zero and 2 hr. in the same experimental situation as that used by Williams and Perin, have indicated that the number of reactions to extinction is a discontinuous function of hours of hunger, the point of discontinuity in the curve (**10**, p. 360) occurring approximately 2 hr. after satiation. Koch and Daniel found a median n value of zero in animals trained to the limit of learning by receiving 70 reinforcements at 23 hr. and being extinguished immediately after satiation. In an unpublished study utilizing 19 Ss, the present author has confirmed the substance of these findings of Koch *et al.*

Drive (D) as a Function of Hours of Hunger (h)

Having determined ${}_S\dot{E}_R = f(h)$ under conditions where number of reinforcements was kept constant but hours of hunger were varied, we may now proceed with the determination of the relationship of drive to hours of hunger. The assumption involved at this point is that the different hunger groups were similar with respect to habit strength, i.e., that habit strength is dependent on the number of reinforcements (N) but independent of drive.

Kendler (**7**) tested this assumption in two experiments in which different levels of drive strength were obtained by varying the degree of nonrewarded thirst drive operating simultaneously with a constant hunger drive. It was found that different drive conditions during learning did not influence the amount of habit strength development as measured under conditions of equal deprivation by trials to extinction and relearning. The results tend to support the validity of the assumption that habit strength is independent of drive.

Because each hunger group of the present study was extinguished at the same number of hours of hunger that it had during training, the obtained data do not provide a direct test of this assumption. A partial test can be made, however, since the data indicate that all groups had approached the limit of habit strength development under the reinforcement conditions in this experiment. At the limit of learning it can be expected that response latencies will reach a stable minimum (**1**, **2**). To show this effect in our data, we arbitrarily

chose trial 72 to be compared with trial 88. Both 72 and 88 were the fourth trials of their respective training sessions. The medians, means, and mean differences for each of the hunger groups are presented in Table V. It can readily be seen that the trials intervening between trials 72–88 produced no marked or consistent decreases in latency. None of the t's of the mean differences was significant beyond the .40 level of confidence.

As previously indicated, the assumption of equal habit strength between the various hunger groups at the end of training will be applied in the following manner. Hull (**5**, p. 226 ff.) has postulated that reaction potential ($_sE_R$) is produced by a function of habit strength ($_sH_R$) in a multiplicative relationship to a function of drive (D), i.e.,

$$_sE_R = f(_sH_R) \times f(D), \qquad \text{(d)}$$

where $f(_sH_R)$ is effective habit strength ($_s\bar{H}_R$), and $f(D)$ is effective drive ($\bar{D}$). When $_s\bar{H}_R$ has a value of 1.0 (**2**, p. 517) at the limit of learning, as is believed to be the case between the groups of the present study at the beginning of extinction, $_sE_R$ practically reduces to $\bar{D}$, since $1 \times \bar{D} = \bar{D}$.

TABLE V

Comparison of Response Latencies on Trials 72 and 88 During Training

Hours of Hunger	No. of *Ss*	Latency in Seconds			
		Median		Mean	
		Tr. 72	Tr. 88	Tr. 72	Tr. 88
3	42	0.990	1.002	1.107	1.055
12	39*	1.088	1.128	1.185	1.263
24	66	0.935	0.934	1.095	1.111
48	36	1.059	0.905	1.162	1.090
72	37	0.973	1.044	1.092	1.057

* One *S* was sacrificed before extinction because of attacks by its cage-mates. The number of *Ss* actually extinguished was 38.

In this case, equation (d) will reduce to,

$$_sE_R = \bar{D}.$$

The quantitative relationship of drive to hours of hunger can now be obtained by substituting $\bar{D}$ for $_s\hat{E}_R$ in equation (4), i.e.,

$$\bar{D} = [37.824 \times 10^{-27.496(1/h)} + 4.001][1 - .00001041\, h^{2.486}]. \qquad (5)$$

Discussion

Theoretically there should be three essentially different methods of determining the relationship,

$$D = f(h),$$

which presumably should yield exactly the same equation: (a) direct quantification of $_sE_R$ at the limit of learning by the Thurstone technique of the paired comparison of response latencies (**1, 2, 6**) of groups differing with respect to h; (b) conversion of the median latencies at the limit of learning into $_sE_R$ by the equation $_sE_R = f(_st_R)$ (**2, 17**); and (c) extinction of $_sE_R$ at the limit of learning and the conversion of extinction responses (n) into $_sE_R$ by the equation $_sE_R = f(n)$.

The third of these alternative methods was the one employed in the present study. The first two methods were not applicable here because significant between-groups differences in latencies were not obtained except on the earliest learning trials (**16**). This was presumably due to the unusual enfeeblement caused by the maintenance procedure of keeping all groups of *Ss* on the same feeding cycle of 72 hr. of hunger followed by 48 hr. of feeding.

The enfeeblement produced by long periods of deprivation is one of the main factors which complicates the determination of hunger drive intensity throughout its entire range. The

effects of inanition have been recognized (**4**, **11**, **18**) but it has been difficult to determine the quantitative relationship of inanition to length of privation. The quantification of the data of the present study, however, resulted in a two-component equation for effective drive as a function of hours of hunger (equation 5). It is tentatively proposed that these components represent drive (D) and an inanition index (ϵ).[4]

The first bracketed component of equation (5) is an exponential growth function of the reciprocal of h, which produces an S-shaped positive curve. This is assumed to be the drive (D) component of effective drive. The second bracketed component of equation (5) is assumed to indicate the effect of inanition (ϵ). For convenience, the power function within this component might be designated as enfeeblement (γ), so that,

$$\bar{D} = D \times \epsilon, \qquad (e)$$

or,

$$\bar{D} = D(1 - \gamma). \qquad (f)$$

Enfeeblement (γ) increases with positive acceleration with increases in number of hours' hunger. Although γ ranges from zero to infinity, its use is arbitrarily restricted to the range zero to one, since $\bar{D}$ is based on absolute zero. Consequently, ϵ will also range from one to zero.

The assumed quantitative relationships of these variables to hours of hunger are:

$$D = 37.824 \times 10^{-27.496(1/h)} + 4.001 \qquad (6)$$

$$\gamma = .00001041\ h^{2.486} \qquad (7)$$

$$\epsilon = 1 - .00001041^{2.486} \qquad (8)$$

[4] In Hullian terminology (**5**), D refers to the relevant primary drive, hunger in the case of the present experiment. Although the non-dominant drives ($\bar{D}$) were presumably in a state of quiescence, $\bar{D}$ may be included in our quantitative expression of D.

TABLE VI

VALUES OF EFFECTIVE DRIVE ($\bar{D}$), DRIVE (D), AND INANITION (ϵ) CALCULATED FROM THEIR RESPECTIVE EQUATIONS

Hours of Hunger	Drive (D)	Inanition Index ($\epsilon = 1 - \gamma$)	Effective Drive ($\bar{D} = D \times \epsilon$)
3	4.001	.9998	4.000
6	4.002	.9991	3.998
12	4.194	.9950	4.173
18	5.124	.9863	5.054
24	6.706	.9719	6.515
36	10.517	.9230	9.707
48	14.115	.8427	11.895
60	17.168	.7260	12.464
72	19.700	.5689	11.207
84	21.802	.3676	8.014
96	23.560	.1186	2.794
101	24.203	.0000	0.000

Values of D, ϵ, and $\bar{D}$ calculated for hours of hunger ranging from 3 to 101 are presented in Table VI for explanatory purposes. It can readily be seen that drive continues to rise with increases in hours of hunger, although effective drive decreases after 60 hr. Also, ϵ decreases more rapidly at the higher values of h than at the lower values. The range in hours of effective drive is limited by the range of ϵ. Whether these theoretical values are essentially correct remains to be seen, but equation (4), from which these values basically originate, provides an excellent fit to the empirical data, as can be seen in Fig. 2.

The weight data of the present experiment indicate that enfeeblement is not linearly related to loss in weight due to food deprivation. Table VII presents the mean satiated weight of each hunger group at the end of training and the mean weight at the time of extinction testing. The per cent values of the former which the privation weights constitute are also shown in Table VII. The percentages decrease with negative acceleration as a

TABLE VII

MEAN BODY WEIGHT IN GRAMS OF THE HUNGER GROUPS AFTER SATIATION AT THE END OF TRAINING AND AT THE TIME OF EXTINCTION TESTING

Hours of Hunger	Weight after Satiation	Weight at Extinction Testing	% of Satiation Weight at Testing
3	303.9	301.9	99.3
12	310.1	295.3	95.2
24	309.2	282.9	91.5
48	314.0	270.6	86.2
72	307.0	255.0	83.1

function of hours of hunger,[5] whereas γ increases with positive acceleration. In the case of such a drive as sex, deprivation may result in little or no inanition (**12**). Thirst, however, may produce strong inanition after relatively short periods of deprivation. Warner (**14**) found that the number of grid crossings decreased after one day of water deprivation. Also, a high mortality rate occurred in rats deprived of water for eight days.

Although the quantitative formulation of drive and inanition is at present tentative, some experimental results on drive stimulus generalization which tend to support the hypothesized nature of effective drive will be reported in an article now in preparation. At the very least, the present study may serve to open the question in a definite, quantitative manner.

SUMMARY

The present investigation was designed to formulate hunger drive as a quantitative construct by determining its functional relationship to hours of food deprivation.

Accordingly, 219 mature male albino rats were trained on a manipulandum response in a modified Skinner box and extinguished by massed trials after 88 quasi-distributed reinforcements. The *Ss* were divided into five groups, and each group trained and extinguished at one of five food deprivation intervals: 3, 12, 24, 48, and 72 hr. of hunger. The number of reactions to extinction was used as the measured manifestation of reaction potential. The experimental findings resulted in the following quantitative relationships:

1. Previously, the relationship of extinction reactions to absolute reaction potential was established under conditions similar to those of the present study. When the equation representing this relationship was applied to the extinction data of the present study, the relationship of absolute reaction potential to hours of hunger was found to be:

$$ {}_{s}\dot{E}_{R} = [37.824 \times 10^{-27.496(1/h)} + 4.001][1 - .00001041\ h^{2.486}]. $$

This equation and those derived from it are viewed as not applicable to h values below 3.

2. Assuming that habit strength (${}_{s}H_{R}$) was equal between the groups at the end of training so that differences in reaction potential would be due entirely to differences in effective drive, i.e., ${}_{s}\dot{E}_{R} = \bar{D}$, $\bar{D} = f(h)$ is expressible as:

$$ \bar{D} = [37.824 \times 10^{-27.496(1/h)} + 4.001][1 - .00001041\ h^{2.486}]. $$

The curve representing this equation has a maximum at approximately 60 hr. Between 3 and approximately 24 hr. it rises with a concave upward curvature, but beyond 24 hr. it becomes convex upward. The curve descends with an increasing acceleration beyond the maximum, reaching absolute zero at $h = 101$.

[5] Per cent of satiated weight (W per cent) as a function of hours of hunger was found to be:

$$W \text{ per cent} = .218 \times 10^{-.01\,h} + .79.$$

3. It is proposed that the two components of $\bar{D} = f(h)$ can be tentatively considered as representing drive (D) and an inanition index (ϵ). The latter construct is 1 minus enfeeblement (γ), so that,

$$\bar{D} = D \times (1 - \gamma).$$

4. The relationship of drive to hours of hunger would then be,

$$D = 37.824 \times 10^{-27.496(1/h)} + 4.001.$$

According to this formulation, D is a positive monotonic S-shaped function of h.

5. Enfeeblement (γ) is positively related to hours of hunger and is considered to be represented by,

$$\gamma = .00001041\ h^{2.486}.$$

Enfeeblement (γ) is a positively accelerated monotonic function of h, increasing from zero to 1. Several of the factors which apparently determine the nature of the relationship of γ to h are age, maintenance conditions, method of measuring learning, coexistent drives, and differences between various drive modalities.

6. The inanition index (ϵ) is $1 - \gamma$, a positively accelerated monotonic function of h, decreasing from 1 to zero.

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MOTIVATION IN LEARNING: X. COMPARISON OF ELECTRIC SHOCK FOR CORRECT TURNS IN A CORRECTIVE AND A NON-CORRECTIVE SITUATION [1]

BY KARL F. MUENZINGER AND ROBERT F. POWLOSKI

The University of Colorado

In previous studies (**1, 4, 5**) it has been shown that electric shock for correct turns after the point of choice has an accelerating effect upon white rats learning a black-white discrimination in a corrective situation. Using a non-corrective situation, where an animal was not permitted to retrace his entry into the wrong alley, Wischner recently (**8**) obtained results which, if stated in terms of reinforcement (**6**), also indicated a slight accelerating effect of shock for correct turns. Since Wischner did not have a corrective situation as a control condition it is impossible to say whether the smallness of the accelerating effect was due to the non-corrective as contrasted with the corrective condition, where the accelerating effect might be expected to be large, or to the particular apparatus used which differed in some respects considerably from the one used by us. The present experiment was therefore designed to compare the effect of shock for correct turns in a corrective with that in a non-corrective situation.

In fairness to Wischner it must be said that his own interpretation of his results is different from the one just presented. His statement that in his experiment "learning of the shock-right animals was retarded rather than facilitated" (**8**, p. 279) is based on differences of what he calls "trials" and "errors" in the no-shock and shock-right groups. As the senior author has pointed out (**6**), Wischner's use of the term "trial" is not the same as that of the Colorado experimenters, and his conclusions are therefore not directly comparable to ours in terms of "trials." In terms of errors or wrong turns his shock-right group was poorer than the no-shock group. But a slight accelerating effect of shock-right is apparent if his results are stated in terms of the number of reinforcements required to reach the criterion of learning, as was done in the reports from the Colorado laboratory. This whole issue has been discussed recently (**6, 8**). Further reference to Wischner's use of the terms "trial" and "error" will be found below.

The present experiment, in addition to being primarily concerned with a further exploration of the effects of shock for correct responses, will also serve the purpose of offering, as Tolman has phrased it, another "controlled comparison" (**7**, p. 5) between non-corrective and corrective procedures in three different situations. The first systematic comparison of this kind was made by Hull and Spence (**2**).

Conditions and Methods of Training

The apparatus, a **T**-shaped discrimination box, is illustrated in Fig. 1. The four doors on each side were of the guillotine type and were raised and lowered by strings leading to *E*'s

[1] The authors wish to thank the Council on Research and Creative Activity of the University of Colorado whose financial assistance made this study possible.

stand. Once an animal was placed at the starting point it was not handled until the day's experimental session was over; the 15-w. light bulbs behind panels of frosted glass were turned on and off by a double-throw switch which also controlled the electric current in the grids on the floors of the two-choice alleys. By means of a separate switch the grid on the same side as the light (shock-right), or the grid on the opposite side (shock-wrong), could be charged or the current could be turned off completely.

The shocking device delivered ten direct current impulses per second, which were of rectangular shape and whose amplitudes could be varied. All references to strength of current are in terms of these amplitudes and do not represent average currents.

Sixty rats, of Sprague-Dawley strain, 60–90 days old, were divided into six groups, three non-corrective and three corrective training groups. Within each set of three groups, one group received no shock, one received shock for wrong turns, and one received shock for correct turns. There were 27 males and 33 females about evenly distributed among the groups.

Since all *S*s had been handled since weaning time they were quite tame. One week prior to experimentation they were gradually adjusted to shorter feeding periods until each *S* was permitted to eat only 45 min. per day. They were fed Purina dog chow supplemented by greens. Water was available at all times. The food in the maze consisted of one cheese crumb for a correct choice.

The preliminary training lasted two days. An animal was placed first in one of the goal boxes and allowed to eat and pass on to the delay chamber. Then it was moved by hand to the other goal chamber. Next it was placed on one of the grids, which were both uncharged, facing the door to the goal box which it was allowed to enter for food and subsequent entry into the delay chamber. This was repeated on the opposite side of the maze, and then once more with the rat farther away from the door to the goal box. The lights in the end box were not turned on. After each daily session throughout the experiment *S* was placed in a feeding cage and allowed to eat for 45 min.

The regular training started on the third day. For four days *S* was given five reinforcements per day, and thereafter ten per day until 100 had been given. Those *S*s who had not reached the criterion of learning by this time were continued until they had done so. The criterion was two successive errorless daily sessions of ten reinforcements each.

The training was light-positive. The sequence of correct turns was R(ight) L(eft) LRRLRLLR for the first and even tens of trials, and LRRLLRLRRL for the odd tens. This procedure of shifting the positive stimulus on the basis of reinforcements is necessary if one is to compare the corrective with the non-corrective situation. Wischner, who did not have a corrective situation, shifted the stimulus on the basis of single choices, whether right or wrong, as did also Hull and Spence.

The *S* was placed in the lower end of the approach alley from where it moved to the point of choice. The doors leading into the choice alleys were open as well as the one in the lighted alley leading into the goal box containing a food tray. In the *corrective* situation all other doors

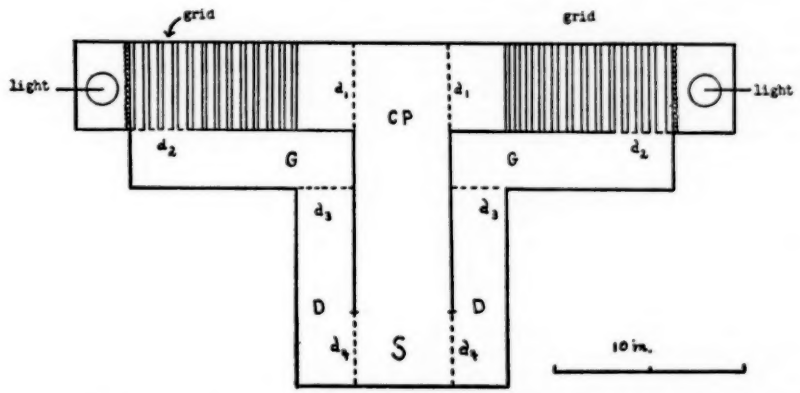

FIG. 1. Floor plan of the apparatus. **S**, starting point; **CP**, choice point; **G**, goal box; **D**, delay chamber; d_1, d_2, d_3, d_4, sliding doors.

were closed. If *S* entered the wrong (dark) alley it had to retrace it. If it entered the correct (lighted) alley it could proceed until it had found the food. The door from this alley was then closed and the one into the delay alley was opened. After consuming the food, *S* would proceed to the delay alley and the door behind him was closed. After 45 sec. the door into the approach alley (S) was opened and closed again as soon as *S* had entered it. The whole cycle would then start over again without handling *S*. The number of times a wrong alley was entered during one cycle was recorded. *S* was considered to have entered an alley if its body exclusive of the tail had passed the door.

In the *non-corrective* situation the doors on the wrong side leading into and out of the empty goal box were open in addition to the doors at the choice point and the one leading from the correct alley into the baited goal box. If *S* chose the correct alley it was treated in the same manner as in the corrective situation. However, if it entered the wrong alley it was permitted to proceed as far as the delay chamber where it was detained for 45 sec. and then permitted to enter the starting alley from where it would move to the point of choice. There was no retracing: *S* could only move forward from start to start. The lights were shifted according to the pattern already mentioned on the basis of successive reinforcements and not successive choices. Thus, *S* could choose the same wrong alley any number of times until it made a correct turn, and it was only then that the pattern of successive right and left lights and food was applied. In this, as in the corrective situation, we have a record of the number of wrong turns between any two reinforcements.

The shock was turned on continuously between two reinforcements beginning only with the second day. On the first choice of the first day no shock was used. In succeeding choices the shock was turned on for 1 sec. after *S* had entered either the wrong or correct alley according to whether it belonged to the shock-wrong or shock-right group. If *S* entered the correct alley the door at the choice point behind it was closed. If *S* entered the wrong alley the door at the choice point was closed only in the non-corrective situation but not in the corrective situation. After about six days it was not necessary to manipulate these doors because *S*s no longer attempted to retrace.

The intensity of the current was near threshold value during the first ten reinforcements, or two days. It was increased on the third and fourth days, and beginning with the fifth day, or after 20 reinforcements, it was left at 100 microamperes for the rest of the experiment.

Terminology

It has been customary to use the term "trial," in the corrective situation, as referring to that division of an experimental sequence in which an animal proceeds from the starting point in a T-maze to the goal box, including any number of entrances into the wrong alley. However, in the non-corrective situation the term "trial" has denoted a single choice whether right or wrong (2, 8, 9). It is obvious that results in the two situations would not be comparable were one to use "trials" with one meaning in one situation and with another meaning in the second situation. This semantic confusion and its consequences has been discussed by the senior author (6). In order to report the present experiment in comparable terms for both situations we shall state the results in terms of correct turns or reinforcements to reach the criterion, and in terms of wrong turns.

The term "error" has previously been used by the senior author and his co-workers in a way which now appears unsatisfactory. It was counted a single error if an animal entered the wrong alley once *or several times* in a single start-to-goal sequence or "trial." The need for comparability between the corrective and non-corrective situations makes such terminology impossible. In this report we shall use the phrase "wrong turn."

Results

One of our concerns is the comparison of learning efficiency in a corrective with that in a non-corrective situation. As can be seen by Table I, the latter requires more reinforcements to reach the criterion and shows more wrong turns than the former. This is true for the three conditions of

TABLE I

Reinforcements to Reach the Criterion
$N = 10$ per group

Conditions	Learning Procedure			
	Corrective		Non-Corrective	
	M	*SD*	*M*	*SD*
No-shock	107	17.4	156	50.0
Shock-wrong	54	10.2	78	14.7
Shock-right	72	13.3	120	39.0

TABLE II

WRONG TURNS BEFORE REACHING THE CRITERION
$N = 10$ per group

Conditions	Training Procedure			
	Corrective		Non-Corrective	
	M	*SD*	*M*	*SD*
No-shock	27.2	5.66	41.9	12.20
Shock-wrong	10.6	3.16	19.2	3.03
Shock-right	17.0	3.32	33.0	6.93

no-shock, shock-wrong, and shock-right. The differences between comparable corrective and non-corrective groups are all significant at the .01 level of confidence.

The *p*-values were obtained by the Festinger technique for skewed samples (**3**) since the total distributions of reinforcements and wrong turns for the 60 *S*s were skewed to the right.

It may be noted that the relative differences between the means as shown by their ratio are smallest in the condition of no-shock (1.56 for reinforcements, 1.54 for wrong turns) and largest for the condition of shock-right (1.92 for reinforcements, 1.94 for wrong turns).

Our major concern is with the relative effectiveness of shock for correct turns, or shock-right, in the non-corrective situation as compared with its previously established function in a corrective situation.

In a corrective situation electric shock for correct turns in a **T**-shaped box presenting a black-white discrimination problem had been found to have a decided accelerating function (**4**, **5**). The present experiment again demonstrates this effect although a differently designed apparatus was used and the animals were handled differently as compared with the apparatus and methods in the previous experiments. The differences in the corrective situation between means of reinforcements and errors of the no-shock and shock-right groups were significant at the .01 level of confidence.

In the non-corrective situation similar differences between no-shock and shock-right conditions were found, but these differences are relatively insignificant, namely at the .05 level of confidence, for both reinforcements and wrong turns.

In other words, in the non-corrective situation the record of the shock-right group approaches that of the no-shock group rather than that of the shock-wrong group as it does in the corrective situation.

The same fact is shown in graphic form in Figs. 2 and 3 which contain the errors per ten reinforcements for the first 100 reinforcements of all groups. In the non-corrective situation the slight superiority of the shock-right group over the no-shock group does not appear until the last 30 reinforcements. In this situation and up to 70 reinforcements the no-shock group made a total of 279 wrong turns and the shock-right group 283, whereas between 70 and 100 reinforcements they made 68 and 27 wrong turns, respectively.

DISCUSSION

1. The superiority of the corrective over the non-corrective situation in regard to learning efficiency, a fact already observed by Hull and Spence (**2**) in a simple **T**-maze, was again demonstrated in the black-white discrimination problem of the present experiment. The reasons for this difference appear to be twofold. First, an animal in the corrective group has to move away from the negative stimulus after a wrong entry and in most cases it will then be

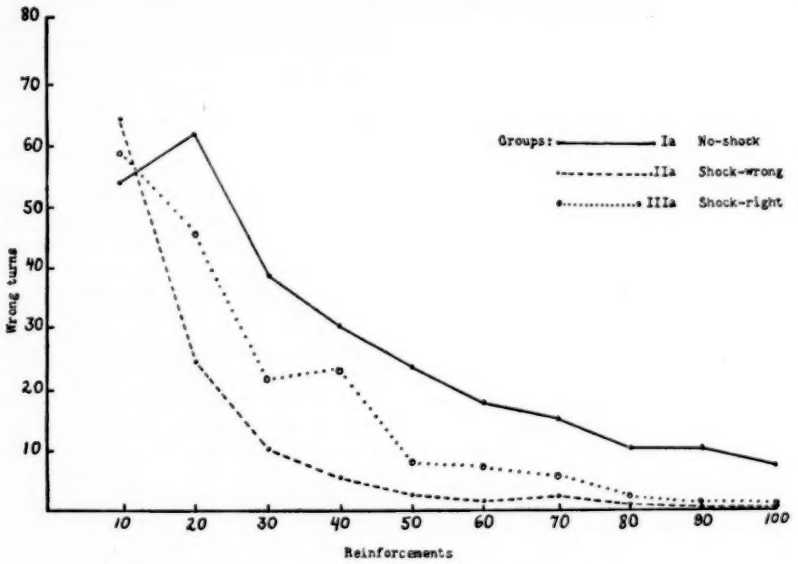

FIG. 2. Wrong turns per ten reinforcements for ten rats in each group in the corrective situation

moving directly towards the positive stimulus in a T-shaped discrimination box. Frustration after a wrong turn guides an animal directly to the positive stimulus and reinforcement unless it turns into the approach alley when it reaches the point of choice whereupon it has to turn around and make another choice. The implication of the term "double-choice" used by Hull and Spence (**2**, p. 127) does not seem to be applicable to all cases of wrong turns followed by right turns. In the corrective situation the number of actual "double choices" was 55 out of 272 wrong turns in the no-shock group, 30 out of 106 in the shock-wrong group, and 52 out of 170 in the shock-right group. If this first reason is valid there should be a difference in learning records obtained in a T-shaped box and in a box with parallel choice alleys.

Second, in the corrective situation reinforcement follows upon frustration in the wrong alley sooner than in the non-corrective situation.

2. Shock-right in the corrective situation again shows the same accelerating effect that had been abundantly demonstrated in previous experiments (**1**, **4**, **5**).

Shock-right in the non-corrective situation produced results which resemble those obtained by Wischner (**8**) in two respects. The shock-right group required fewer reinforcements to reach the criterion than did the no-shock group (**6**, p. 202) although the differences were not significant, or only slightly so, in our experiment. Second, there was a decisive drop in wrong turns "when the animals finally begin to learn," as Wischner puts it (**8**, p. 279), and as also exhibited in Fig. 3. In another respect our results differed from Wischner's. The total number of wrong turns in our experiment was slightly less in the shock-right group than in the no-shock

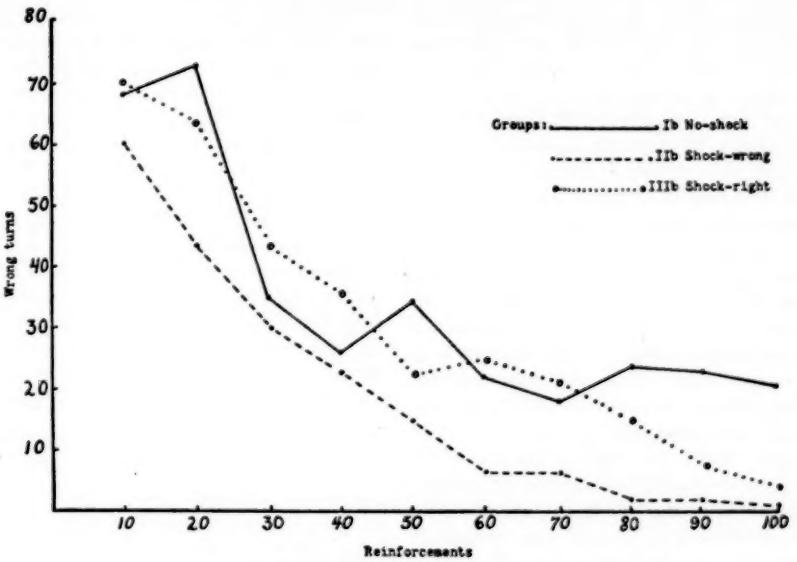

FIG. 3. Wrong turns per ten reinforcements for ten rats in each group in the non-corrective situation

group, while in Wischner's experiment the relation was reversed. In our experiment the two groups, shock-right and no-shock, have practically the same number of wrong turns up to 70 reinforcements. It is the decisive drop in wrong turns after this point that favors the shock-right group in total number of wrong turns, although the difference is relatively insignificant, i.e., only at the .05 level of confidence. It is possible that the discrepancy between Wischner's and our results in terms of wrong turns was caused by his shifting of the right cue on the basis of single choices whether right or wrong, whereas we shifted the light on the basis of right choices in order to produce comparability with the corrective situation. The difference in apparatus—Wischner's choice alleys were side by side and not opposed along a straight line as were ours—may also have contributed to the discrepancy.

3. Compared with shock-right in the corrective situation, shock-right in the non-corrective situation has a very small accelerating effect in terms of both reinforcements and wrong turns to reach the criterion. The corrective shock-right and no-shock groups differ at the .01 level of confidence while the parallel non-corrective groups differ only at the .05 level in terms of reinforcements and wrong turns.

In order to account for the decrease, if not apparent absence, of the accelerating effect of shock-right in the non-corrective situation we postulate that shock after the point of choice whether for right or wrong turns in a visual discrimination box has an accelerating function in both situations, non-corrective as well as corrective, but in addition it also has an avoidance function which may under certain circumstances mask the accelerating effect. Shock after the point of

choice makes the animal more sensitive to the cues to be discriminated, but it also creates a tendency in an animal to avoid the alley in which it has been shocked. If it should be possible to train an animal to adapt itself to and thus overcome the avoidance effect it might then exhibit the accelerating effect more clearly.

In the corrective situation the frustration in the wrong alley and subsequent retracing towards the positive cue seems to counteract the avoidance tendency towards the shock-producing correct alley thus allowing the accelerating effect to assert itself. In the non-corrective situation on the other hand the full avoidance effect is present until the animal has somewhat adjusted itself to it when the accelerating effect will gain preponderance.

Summary

The purpose of this study was to compare the effect of electric shock for correct choices in learning a discrimination by the corrective and non-corrective methods.

Six groups of ten albino rats each were trained in a black-white discrimination with hunger-food motivation. Three of the groups were trained in a corrective situation and under conditions of no electric shock, shock for wrong turns, and shock for correct turns, respectively. The other three groups were trained under the same three conditions but in a non-corrective situation where no retracing after a wrong turn was permitted.

In terms of reinforcements to reach the criterion of learning and wrong turns before reaching it each corrective group was significantly superior to its equivalent non-corrective group.

The shock-right group, with which this study was principally concerned, was significantly superior to the no-shock group in terms of reinforcements and wrong turns in the corrective situation, but only slightly so in the non-corrective situation. The accelerating effect of shock was strongly manifest in the first and less so in the second situation.

These differences are attributed to certain advantages of the corrective over the non-corrective situation, and to the avoidance effect of shock which seems to be counteracted by the advantages of the first situation.

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STUDIES OF DISTRIBUTED PRACTICE: I. THE INFLUENCE OF INTRA-LIST SIMILARITY IN SERIAL LEARNING

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The basic assumption of the McGeoch-type differential-forgetting theory is that during an interval of rest following learning, the erroneous responses, being weaker than correct associations, drop out (are "forgotten") more rapidly than correct responses (**9**, p. 142 f.). The Gibson theory of verbal learning, on the other hand, provides a postulate which is directly contrary to the assumption of the differential-forgetting theory (**3**). Gibson's theory states that a fundamental manipulable variable which causes rate of learning to vary is intra-task similarity. Intra-task similarity operates to influence learning via stimulus generalization. Generalization may be indexed by errors which occur during learning, and these errors have been shown to first increase and then decrease in frequency as learning proceeds to mastery (**2**, **4**). But, an additional postulate in Gibson's theory is that: "After the cessation of practice, differentiation will decrease over a period of time, leading to an increase ('spontaneous recovery') of generalization" (**3**, p. 206). Thus, according to this theory, errors should *increase* in number following a period of no practice.

The differential-forgetting theory has been used to explain the fact that distributed practice leads to more rapid learning than does massed practice. The rest intervals, according to the theory, allow erroneous response tendencies to drop out more rapidly than correct response tendencies, resulting in a net increase in performance following the interval. This theory has received very little confirming evidence in modern studies of verbal learning. McGeoch identifies remote associations as one type of error which drops out with rest intervals, yet Wilson in two studies has failed to find this to be the case (**12**, **13**). McClelland's three studies of verbal discrimination learning also have failed to support the differential-forgetting theory (**6**, **7**, **8**).[1]

Only one study of massed vs. distributed practice in verbal learning is known to have made an error count in such a fashion that a direct test of the differential-forgetting theory can be made. However, this study by Patten (**11**), was inconclusive. Using serial lists of nonsense syllables, with a 2-min. rest interval between each trial, Patten obtained the customary facilitation in learning by distributed practice. In terms of mean number of errors per trial, the distributed condition did not differ from the massed condition. However, Patten did observe that the nature of the errors was different in the two conditions. In general, backward associative errors and long forward anticipatory errors (very remote associations when calculated by serial position) were significantly fewer in number in the distributed than in the massed condition.

[1] McClelland's studies were concerned with reminiscence. However, he attempted to discover if incorrect responses were forgotten more rapidly than correct responses. He was unable to find confirmation for the differential-forgetting theory, but his evidence was not necessarily opposed to the theory.

The distributed condition, on the other hand, produced significantly more short forward anticipatory errors than did the massed learning. Patten makes no attempt to explain these findings. But, the fact that he did not find that there were *fewer* errors per trial with distributed practice casts doubt on the differential-forgetting theory. These studies, considered overall, provide little evidence to support the differential-forgetting theory, but neither do they provide a clear-cut refutation of it.[2]

Gibson's generalization theory makes no attempt to account for the greater effectiveness of distributed over massed learning. Indeed, without additional assumptions, the theory probably cannot handle the facts. Nevertheless, the theory has been found useful in understanding other facts of verbal learning, including transfer and forgetting. And, since it does make specific predictions concerning errors in learning, its implications should be explored for their possible aid in arriving at a more satisfactory theory of distributed practice than is now available. At least one fact is clear; the Gibson theory makes a prediction of a greater number of errors in performance following a rest interval while the differential-forgetting theory makes an assumption of fewer errors following a rest interval.

The present study is the first in a series by which it is hoped that certain theoretical issues involved in attempts to explain the influence of distributed practice in verbal learning and retention will be clarified. These papers will not be concerned directly with the phenomenon of reminiscence, but if facilitation by distribution and reminiscence have a common explanatory base, as is commonly believed, it may be possible to provide information which will help explain reminiscence.

In this first study we have made an examination of errors in the serial learning of adjectives by massed and distributed practice. In three conditions, items of low similarity have been used, and, to maximize error frequency, three additional conditions of high similarity have been employed. From these conditions we should be able to determine two facts: (a) whether or not intra-list similarity is a variable in distributed practice, and (b) whether the errors which occur during learning follow the expectations of a differential-forgetting theory, or of the Gibson theory, or of neither theory.

Procedure

Materials.—The serial lists of adjectives were made up of 14 items each. These lists are reproduced below. The low-similarity lists consist of 14 of the 16 items in lists constructed by Melton (10), while the high-similarity lists were constructed by Buxton (1) for use in his studies of reminiscence.

Low Intra-List Similarity

A	B	C
worldly	inform	waggish
fiery	pretty	spicy
blotchy	hybrid	chagrined
hostile	flaming	rounded
fiscal	shopworn	dusky
swarthy	guilty	plastic
agog	warlike	boorish
crumbling	adroit	discreet
flashy	modern	hairy
sallow	equal	sterile
puny	bloated	elfish
irksome	fretful	faithful
vulgar	neuter	noxious
bearded	tiresome	agape

[2] The Yale version of the differential-forgetting theory as outlined in Hull *et al.* (5) makes no specific predictions concerning overt errors, and on this point must be distinguished from the McGeoch-type theory. In Hovland's long series of studies which form the empirical base of the Yale differential-forgetting theory, no report of overt errors is given. A great deal of data are given on "number of failures" but these include failures to respond as well as overt errors.

High Intra-List Similarity

D	E	F
elated	clever	painful
gleeful	tricky	nasty
carefree	adept	shocking
jolly	cunning	tragic
laughing	wily	harmful
happy	agile	fearsome
pleasant	cagey	evil
festive	skillful	horrid
sunny	foxy	dreadful
blissful	artful	wicked
genial	expert	ugly
smiling	crafty	filthy
cheerful	nimble	hateful
hearty	shifty	depraved

These lists were presented by a Hull-type memory drum at a 2-sec. rate, with an asterisk at the head of the list as the anticipatory signal for the first word.

Conditions.—Three conditions of spacing were used for both the high- and low-similarity lists. These conditions involved 2-, 15-, or 30-sec. between trials. The 2-sec. condition represents massed practice and the other two distributed practice. The three conditions for the dissimilar lists will hereafter be called 2-D, 15-D, and 30-D, and the three conditions for the similar lists will be called 2-S, 15-S, and 30-S. The lists were always learned to a criterion of one perfect trial.

The six conditions appeared equally often at each stage of practice, and equally often with each list of a given extreme of similarity. Each *S* served in all six conditions. For the first 12 *S*s the lists appeared in the order DABEFC, and for the second 12 *S*s the order was ADEBCF. With only three conditions for each extreme of similarity, six *S*s are needed to counterbalance; hence, for the 24 *S*s, four completely counterbalanced blocks were used. This procedure allows direct comparison of results of similar lists against dissimilar lists for all conditions without bias as a function of practice effects, and also direct comparison of conditions for a given extreme of similarity without bias from differential practice effects or differences in list difficulty.

Two practice days were given before *S* started the six experimental conditions. On the first practice day 12 *S*s learned a similar list by distributed practice and on the second day they learned a dissimilar list by massed practice. For the other 12 *S*s these conditions were reversed.

The interval between each trial on distributed conditions was filled with color naming from a board at the rate of 80 colors per min. as timed by a metronome.

No more than two days elapsed between two successive conditions, with only one condition being run each day.

Results

Learning.—The mean number of trials required to learn under each condition is shown in Table I. Distributed practice with dissimilar lists (15-D, 30-D) did not produce statistically significant faster learning than did the massed condition (2-D). The mean difference between 2-D and 15-D is 2.12 ± 1.34 trials, which gives a t of 1.58. With 24 *S*s, a t of 2.07 is necessary for significance at the .05 level of confidence. The t for 2-D vs. 30-D is 1.55. Thus, while the mean differences favor distribution of practice, these differences do not meet statistical requirements of reliability.

Learning of the similar lists is significantly benefited by distributed practice. The mean difference between 2-S and 15-S is 4.25 ± 1.26 trials, which gives a t of 3.37. The t for 2-S vs. 30-S is 3.18. Both indicate differences which are significant beyond the .01 level of confidence. It is to be noted that 15-S and 30-S resulted in almost the same number of trials to learn. The learning curves shown in Fig. 1 indicate that 30-S may be slightly superior to 15-S. Nevertheless, the fact that these two conditions gave results so highly comparable, indicates that longer intervals

TABLE I

Learning of Similar and Dissimilar Lists as a Function of Massing and Distribution

Condition	M	σ_M
2-D	15.08	1.26
15-D	12.96	1.06
30-D	13.21	1.22
2-S	21.67	1.68
15-S	17.42	1.68
30-S	17.00	1.16

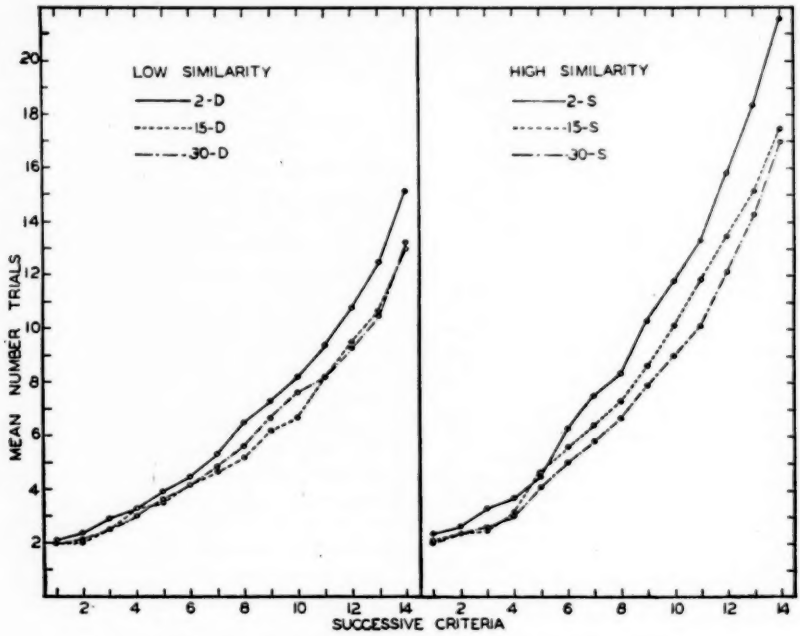

FIG. 1. Learning of serial lists of adjectives as a function of intra-list similarity and length of interval between trials

between trials probably would not provide additional facilitation and might even result in slower learning.[3]

One final fact in the learning scores should be noted. Under comparable conditions the high-similarity lists took longer to learn than the low-similarity lists. This finding is in conformance with Gibson's theory (**3**) and her experimental findings with nonsense forms (**4**).

Errors during learning.—Since a different number of trials was required to learn the lists for the various conditions, the frequency of errors was calculated on an error-per-trial basis. To obtain the values given in Table II, the total errors made in learning each list was divided by the number of trials required to learn. This was done for each *S*, and the mean of the 24 *S*s determined.

It may be pointed out first that the D conditions produced very nearly the same error frequency. None of

[3] In an unpublished study by Albert Elkin it was shown that a 2-min. interval introduced after every second trial did not result in facilitation in learning high-similarity lists. The lists were two of the three used in the present study.

TABLE II

MEAN NUMBER OF ERRORS PER TRIAL IN LEARNING UNDER EACH CONDITION

Condition	*M*	σ_M
2-D	1.15	.15
15-D	1.02	.13
30-D	1.15	.17
2-S	1.33	.14
15-S	1.60	.22
30-S	1.70	.17

the differences between these three means approaches statistical significance. However, not only did the S conditions produce a greater number of errors than the D conditions, but the greatest frequency of errors occurred on the distributed conditions with the highly similar lists. The mean difference between 2-S and 15-S gives a t of only 1.42, but the difference between 2-S and 30-S, $.37 \pm .12$ errors, produces a t of 3.08. This indicates that the 30-sec. rest interval introduced after each trial produced a greater number of errors than did the 2-sec. interval, even though faster learning took place under the 30-sec. condition. In short, with highly similar lists, *rate of learning and number of errors are directly related.*

Frequency of errors as a function of stage of learning is shown in Fig. 2 for all six conditions. The method used in deriving these curves may need clarification. For each S the number of errors made in going from one performance criterion to the next was determined. Thus, we recorded the number of errors made in going from one correct response to two correct responses; from two to three, etc. If S jumped a criterion, say, for example, from four correct responses to seven correct responses, the total errors made was divided by the number of items attained; or, in the above illustration, three. The error value assigned for the criterion was the resultant quotient. These errors-to-criterion values were derived for each S. Then, in order to equalize conditions

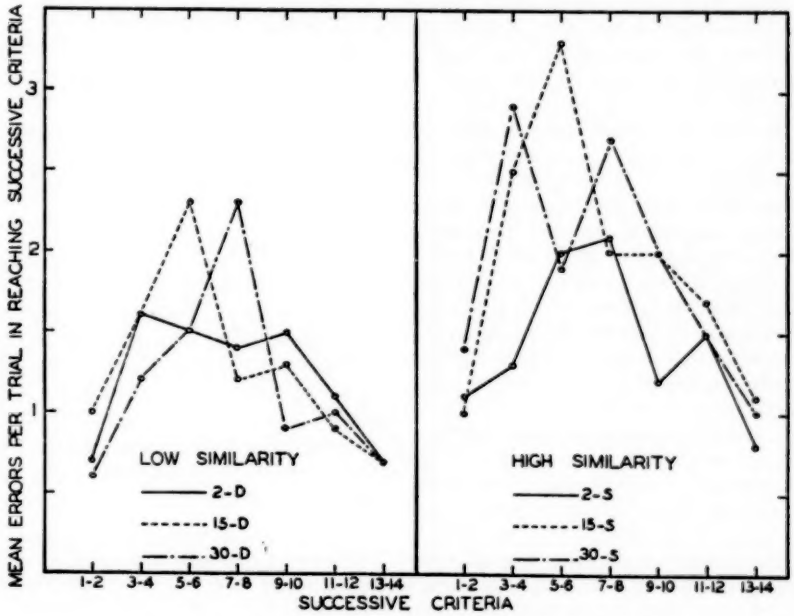

FIG. 2. Errors in learning serial lists as a function of intra-list similarity and length of interval between trials

TABLE III

MEAN ERRORS PER TRIAL DURING FIRST HALF AND SECOND HALF OF LEARNING AS A FUNCTION OF DEGREE OF REMOTENESS

Cond.	First Half				Second Half			
	One Degree		Two or More		One Degree		Two or More	
	M	σ_M	*M*	σ_M	*M*	σ_M	*M*	σ_M
2-S	.60	.12	.73	.07	.60	.08	.35	.05
15-S	.57	.12	1.06	.20	.61	.09	.50	.06
30-S	.76	.13	.89	.15	.72	.08	.50	.07

for trial differences, the error values were divided by the number of trials required to go from one performance criterion to the next. Finally, successive pairs of criteria were combined to give some stability to the curves.

These curves in Fig. 2 show that, in general, as learning proceeds, the frequency of errors first increases and then decreases. This is in accord with previous findings (**2**, **4**). The curves also substantiate the implications of the error values given in Table II. With similar lists, the distributed conditions yielded the greatest number of errors; with the dissimilar lists little difference is observable among the conditions. However, for all distributed conditions, there is a greater peak to the curves than for the massed conditions. These peaks will not meet statistical requirements of significance but their consistent appearance with the distributed conditions may lend some reliability to them.

Another analysis of errors has been made in terms of the nature of the error and its frequency during the first half of learning as opposed to the second half. The mean errors per trial for each *S* was determined for the trials required to attain seven correct responses; these are called the errors for the first-half of learning in Table III. The mean number of errors per trial in learning beyond seven correct constitutes the second half of learning. One-degree errors were those in which *S* anticipated a response one position too soon; erroneous anticipations of more than one step were pooled to form the classification in Table III labelled "two or more." The following facts summarize this table:

1. There is no reduction in one-degree errors between the first and second half of learning.

2. There is a marked reduction in frequency of remote associations ("two or more") between first and second half of learning. Roughly, this reduction is the same for all conditions.

3. In general, the distributed conditions showed a greater frequency of errors of all kinds than did the massed. For 2-S vs. 30-S this is true in every comparison. Thus, these results do not support Patten's findings (reported earlier in this paper) that remote associations occurred with less frequency in distributed than in massed conditions.[4]

[4] Backward errors for similar lists and errors on dissimilar conditions did not occur with sufficient frequency to allow the breakdown given in Table III. That is, there was a marked skewing of the distributions with many *S*s showing zero mean errors of a given type for a given half of learning.

Discussion

The present conditions have shown that when distributed practice significantly facilitated learning (high-similarity lists), more errors occurred during distributed learning than during massed learning. Rate of learning and frequency of errors were directly related. In the low-similarity lists, where learning was not appreciably influenced by distributed practice, there was little difference in error frequency for the massed and distributed conditions. Thus, no evidence is found to support the McGeoch differential-forgetting theory and results with high-similarity lists flatly oppose it. This evidence, considered in conjunction with the other data reported in the introduction to this paper, suggests that it may be worthwhile to seek a new theoretical approach to explain the facilitation in learning produced by distributed practice. Before discussing such possibilities, however, note must be taken of the discrepancy between our results and those of Patten's (11).

As indicated earlier, Patten found no difference in the overall frequency of errors produced in learning serial lists of nonsense syllables by massed and distributed practice. Since he found that distribution facilitated learning, we must conclude immediately that it is not essential that there be a greater number of errors by distribution for facilitation to occur. Yet, in view of the fact that Patten found differences in the nature of the errors occurring under massed and distributed practice, a consideration of the processes which produce errors may eventually lead to an understanding of the nature of the theoretical constructs which will be necessary to account for facilitation by distributed practice when it does occur. It is not the intent at the present time to attempt to account for the discrepancies in Patten's results and the present results. Several gross differences in the experiments precludes this. It may be said, however, that it will be shown in subsequent papers that whenever distribution is found to facilitate learning, a greater number of errors has occurred than under the corresponding massing conditions. And, when learning by distribution has not facilitated learning, no difference in error frequency has been found. These facts suggest that tentatively, at least, errors may reflect directly the processes producing facilitation by distribution, and that they are not ancillary products which are unreliably related to such processes. Therefore, the remainder of this discussion will be concerned with alternative ways by which error processes might produce facilitation when practice is distributed.

As previously discussed, Gibson's theory of verbal learning predicts a greater frequency of errors for distribution than for massing. The theory states that errors are produced by intra-list generalization tendencies. A part of the process of learning requires that these generalized tendencies be reduced, i.e., items must be differentiated. With a rest interval, these generalized tendencies would recover, being evidenced by a greater error frequency with rest than without. The next step, however, that of stating how learning could be facilitated by such processes, is not indicated. One might even suspect that the theory would predict slower learning by distribution because of the greater number of error tendencies. But this need not be the case. It might be possible that error tendencies which recover with rest and are then inhibited (extinguished?) on the

next learning trial, etc., would eventually be less interfering than would errors which are merely "suppressed" by the massed practice. If this were the case, however, we would expect the frequency of errors to fall off rapidly late in learning by distributed practice. At least, they should fall off more rapidly than errors under massing. The curves we have presented and the first-half, second-half error analysis have shown this not to be the case for our data.

Another alternative is to accept Gibson's theory concerning the cause of greater frequency of errors by distributed than by massed practice, and then assume that more rapid differentiation between right and wrong responses takes place as a consequence of their overt occurrence. But again, such an assumption would demand that the reduction in error frequency in the later stages of learning be more rapid with distributed than with massed practice. Our data do not show this to be the case. Gagné (2) has recently presented data which he interprets as supporting the idea that overt errors provide the means for rapid differentiation. His data should be examined in some detail.

Gagné was not concerned with effects of distributed practice, but rather, with learning as a function of temporal contiguity among similar stimuli (figures) in paired-associate lists. He found that when similar items were placed together in a list, learning took place more rapidly than if the items were separated by other dissimilar items. The following contains Gagné's basic conclusions:

> When the closely similar items are grouped together, a high degree of confusion, exhibited by the occurrence of a large number of overt errors, takes place toward the beginning of learning. This is followed by a period of rapid reduction in the number of errors as learning continues. In contrast, when the highly similar items are separated in sequence by dissimilar items, the number of overt errors increases more slowly, and decreases more slowly after reaching a maximum, as learning proceeds. (2, p. 70).

Gagné then proceeds to apply the reasonable idea that greater differential reinforcement, produced by greater error frequency, leads to more rapid learning in conditions where similar items are contiguous than in other conditions. An inspection of Gagné's data, however, does not reveal clear-cut differences in rate of error reduction for the different conditions. The lists were presented for 14 trials. At the end of that time the group with similar items closely contiguous had learned significantly more than the other groups. The error frequency was also significantly less on the fourteenth trial for this condition than for the other conditions. But, *at comparable stages of learning* there is no demonstration of significantly fewer errors for this condition. That is, by the tenth or eleventh trial the group with similar items contiguous had learned as much as the other groups had at the end of the fourteenth trial. Thus, differences in error frequency at comparable stages of learning would be far from significant statistically. In short, while the idea that greater frequency of errors allows for more rapid differentiation, hence more rapid learning, is an attractive one, the error-reduction curves do not support it in either Gagné's data or in ours.

A final possibility to be suggested is that when learning is by massed practice a form of generalized inhibition accrues which reduces the frequency of both correct and incorrect responses. Distributed practice prevents such inhibition from accumulat-

ing, the effect being for more responses to be made, both more correct and incorrect. Any factor which made learning more difficult would increase the amount of generalized inhibition. Thus, length of list, speed of presentation, and intra-task similarity would all be directly related to difficulty. And, the facts are, that all of these conditions do determine whether distribution will facilitate learning (as compared with massed practice). If we understand McClelland (7), this is essentially what he means by his performance-inhibition theory. The theory, as it now stands, lacks much-needed specificity for predictive purposes, but it is an approach which should receive additional attention.

Our conclusion is that no current theories of distributed practice adequately account for our data. Furthermore, we are unable, on the basis of our present data, to advance an acceptable theory.

Summary

The present experiment was designed to (a) ascertain if intra-list similarity is a variable in determining whether or not distribution of practice facilitates serial learning, and (b) test whether the McGeoch differential-forgetting theory or the Gibson hypothesis most adequately predicts error frequency in serial lists learned by distributed practice.

Lists with low similarity of items were learned with 2-sec., 15-sec. or 30-sec. rest between trials. Lists of high similarity were learned under the same conditions. The items were presented at a 2-sec. rate, with 24 Ss serving in all six conditions. The results show:

1. The learning of lists of low similarity was not appreciably facilitated by 15 sec. or 30 sec. between trials.
2. Lists of high similarity were learned significantly faster when 15-sec. or 30-sec. rest was given after each trial than when 2-sec. rest was provided.
3. Lists of low similarity were learned much more rapidly than those of high similarity.
4. A greater frequency of errors occurred with distributed practice than with massed practice in learning the high-similarity lists. This was interpreted as confirmation of Gibson's theory of verbal learning and in contradiction to the McGeoch differential-forgetting theory.

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September 14, 1950)

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EFFECTS OF TOTAL LIGHT FLUX ON CRITICAL FLICKER FREQUENCY AFTER FRONTAL LOBE LESION [1]

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This study is concerned with two problems: (a) The effects of frontal lobe lesions, in man, upon the fusion threshold for flickering light (i.e., the critical flicker frequency, or CFF); and, (b) the effect on CFF of varying both target area and light intensity (and hence total light flux). These two problems will be discussed separately.

Effects of Frontal Lobe Lesions on CFF

Permanent reductions in CFF have been previously demonstrated in patients with homonymous field defects (**2, 14, 15, 16, 17**). We have interpreted these reductions as the direct result of injury to the higher visual pathways, i.e., the geniculo-calcarine system and the optic tracts (**2, 4, 17**). One might question this interpretation, however, since others have reported that a reduction of CFF occurs after lesions in the frontal lobes (**9, 18**). If a reduced CFF is a permanent result of frontal lesions, then one cannot attribute the depression found in patients with homonymous field defects exclusively to damage of higher visual pathways.

To clarify this situation Battersby (**2**) recently undertook a comparative study of CFF in patients with long-standing lesions of either the frontal or occipital lobes. In this study, monocular CFF was measured at the fovea, and at various degrees of arc in the periphery. Two target sizes (2° and .5° of visual angle) were employed; the light intensity and light-to-dark ratio were held constant. Confirming the previous reports of Teuber and Bender (**16, 17**), a significant depression in CFF was found after occipital lobe injury; however, no depression was found after frontal lesions.

In view of these negative results one could conclude either that CFF is not regularly and permanently decreased after frontal lobe injury, or that a permanent decrease exists, but cannot be detected unless one employs special conditions of testing. Such special conditions might include the size and intensity of the target. Thus, if one varies the size (area) and intensity of the flickering patch over a sufficiently wide range, a depressed CFF may be demonstrable after frontal lesion. The present study is concerned with this possibility.

Effects of Area and Intensity on CFF

In the normal *S* the effect of target size on the fusion threshold has been studied by several experimenters. Hecht and Smith (**13**) reported that only a small increase in CFF occurred when the area of a directly fixated target was increased ninefold. Within the limits of .35° to 6° of visual angle, Granit and Harper (**8**) found a linear relationship between CFF and log target area; the slope and *y*-intercept value of this function varied directly with the intensity of the light source, and the location of the target in the visual field. Brown

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(5) confirmed the Granit-Harper function for peripherally placed targets ranging in size from .39° to 13.6° of visual angle. One can conclude that a linear function exists between CFF and log target area. The characteristics of this function vary with light intensity and location in the visual field.

Considerable information exists on the effects of light intensity upon CFF in the normal *S*. The earlier studies showed a linear relationship between the fusion threshold and light intensity [the "Ferry-Porter Law," see Bartley (1) for summary]. More recently, both Hecht (11, 12), and Crozier (6, 7), and their collaborators, have demonstrated that this relationship is actually sigmoid in shape. A single curve is obtained for foveally fixated targets, a duplex curve for peripherally placed targets ("rod" and "cone" curves). In general, the conclusion can be drawn that CFF, in the normal *S*, is raised by increases in either target area (the area effect) or target intensity (the intensity effect).

In all of the experiments quoted, the area effect has been studied at a fixed intensity level, as measured in terms of foot-candles per unit area. Under these conditions, an increase in target area results in an increase in total light flux (area × intensity). One does not know, therefore, whether the increase in CFF in these studies is due solely to the greater area of the target or to the concomitant increase in total light flux.

We can now restate the problems to be studied as follows: (a) Do patients with loss of frontal lobe tissue show any depression in CFF when target size and light intensity are varied independently over wide ranges? (b) What are the relative roles of target area and total flux in determining CFF?

Method

Apparatus.—A Kodak slide projector (Model 1A) was used as a light source. This projector contained a 150-w. General Electric bulb and was powered by the 110 AC house current. An episcotister mounted in front of the projector produced flicker by periodically interrupting the beam of light. A variable-speed motor was employed to rotate the episcotister; the velocity of this motor was thyratron-controlled. The speed of the episcotister was stroboscopically calibrated against the milliamperes of current flowing through the thyratron circuit, and the resulting values corrected to read in terms of cy/sec. (since a two-sectored disc was used). From several such calibrations throughout the course of the study, the reliability of measurement was calculated as ±.5 cy/sec. The light-dark ratio (i.e., the ratio of the open to closed segments of the episcotister) was maintained at 1.

The flickering beam of light from the projector was focused upon a translucent screen. When *S* was seated on the side of the screen opposite the projector, he perceived either a flickering or a steady patch of light, depending upon the rate of light intermittence and other factors. The size of the test patch was determined by placing brass squares in the slide rack of the projector; each square contained an accurately drilled circular hole in its center. Three such targets were used. In order of size they produced on the screen circular patches of light which subtended 6°20′, 2°0′, and 38′ of visual angle. The actual areas of the circular test patches in square feet were .431, .041 and .004, respectively. The distance from the test plate to the translucent screen was equal to the distance from the screen to *S*'s eye, 2 meters in all cases.

The intensity of the light on the screen was varied by placing neutral density Wratten filters into an attachment in front of the projector. Three intensity levels were employed: 3.61, .38, and .03 e.f.c./ft.^{2} The intensity of the light which diffused through the screen surrounding the various test patches was measured. At the highest intensity level of the test patch (3.61 e.f.c./ft.^{2}) the surrounding field intensity was .03 e.f.c./ft.^{2} This light in the surround was of a dark blue color and was uniformly distributed over a circular area with a radius of 18 in. from the center of the test patch. At the two lower levels of target intensity, the surrounding field values were so low that they could not be measured with the Macbeth Illuminometer.

A screen containing a rectangular aperture was placed directly in front of *S* to eliminate diffusion of light throughout the room. The

opening in the screen was large enough to permit *S* to see the test patch with either eye separately, or with both; constant checks were made to insure that *S* always viewed the target with both eyes. Under these conditions, *S* saw a rectangular-shaped segment of the translux screen, about 36 in. by 24 in., which appeared black and contained a circular light patch in its center. A chin rest and response key were placed within the screened enclosure surrounding *S*. Figure 1 presents the general schema of the experimental situation.

Subjects.—Two groups of 16 *Ss* each were tested. Group A consisted of normal *Ss* selected from the authors' acquaintances, Group B of patients with frontal lobe lesions. The normal group was composed of hospital staff; specifically, residents in neuropsychiatry, or experimental psychologists and neurologists working in the laboratory. All normal *Ss* were males of an age and visual acuity directly comparable to the pathological group. It should be noted, however, that the educational and social background of the normal *Ss* differed from that of Group B, the brain-injured group.

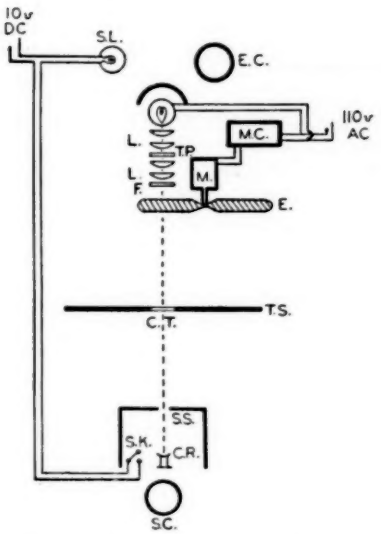

FIG. 1. Schema of the experimental situation. *S.L.*—signal light; *E.C.*—*E*'s chair; *L.*—lenses; *T.P.*—target plate; *F.*—filter; *E.*—episcotister; *M.*—motor; *M.C.*—motor speed control; *T.S.*—translux screen; *C.T.*—circular target; *S.S.*—*S*'s screen; *S.K.*—*S*'s signal key; *S.C.*—*S*'s chair; *C.R.*—chin rest.

TABLE I

TOTAL LUMINOUS FLUX (IN LUMENS/FT.²) OF TARGET AREA AND INTENSITY COMBINATIONS

Target Intensity in e.f.c./ft.²	Target Area		
	.431 Ft.²	.041 Ft.²	.004 Ft.²
3.61	1.556	0.148	0.014
0.38	0.164	0.016	0.001+
0.03	0.013	0.001+	0.0001+

Group B consisted of patients in whom there was evidence of loss of frontal lobe tissue. They were drawn at random from veterans of World War II who had suffered penetrating head injuries due to gunshot or shrapnel. These patients formed part of a larger group of brain-injured men who were studied extensively by the Head Injury Unit of this laboratory. Their Veterans Administration folders, containing skull x-rays, surgeon's notes, and a history of the trauma, were carefully reviewed to establish that a definite loss of frontal lobe tissue had been incurred. No patient was included who could not meet this criterion. The patients in this group were tested between four to six years after their injury. All were able to give reliable and consistent judgments of flicker or fusion during the experimental session. Head phones were worn by all *Ss* during testing to minimize auditory cues from the apparatus producing the flickering light.

Variables.—Three sets of independent variables were employed in this study. As described above, the intensity of the light emitted by the target was varied in three approximately logarithmic steps; the three target areas were similarly varied. In consequence, the nine possible combinations of target area and intensity can be expressed, in terms of their total luminous flux (in lumens/ft.²), as shown in Table I.

The matrix in Table I shows that the total light flux increases in a roughly logarithmic manner as one reads the table along the diagonal from the lower right-hand cell upward and to the left. Similarly, it can be seen that the three cells lying above and to the right of the diagonal contain approximately the same light flux as the corresponding cells to the left and below the diagonal. With this disposition of stimuli, one can compare the fusion thresholds obtained with target-intensity combinations which possess the same total light flux, but which differ in the concentration of light energy (the density in terms of e.f.c./ft.²), and in the size of the test patch.

The third variable investigated was the effect of lesions in the frontal lobes upon CFF. More specifically, it was possible to determine whether such lesions could produce either a general depression in CFF, or a depression specific for a particular target size or intensity.

Procedure.—The order of exposure of the nine combinations of area and intensity was randomized for each *S* according to a prearranged plan. This random selection was accomplished by constructing a 9 × 16 table (9 area-intensity combinations for 16 *S*s per group), and assigning a different rank order of target presentation to each *S* within each group. Both groups received the same order of target presentations. One can assume, therefore, that the group mean results are equally weighted with respect to practice effects or dark adaptation artifacts.

The *S*s were seated behind the translucent screen, the head immobilized in the chin rest, and the general nature of the transition from flicker to fusion and fusion to flicker carefully explained. With the room darkened, *S*s were then given one trial run in ascending (flicker to fusion) and descending (fusion to flicker) order. The *S*s were told to respond by depressing their key the first time *all* flicker disappeared, and again the first time *any* flicker reappeared. All *S*s were instructed to maintain fixation on the center of the targets. Following the trial run, six alternate ascending and descending values of CFF were taken for each of the nine area-intensity combinations. All data for each *S* were taken in one session, which usually lasted about an hour.

Results

The relationship between log target area and fusion frequency for normal *S*s and for patients with frontal lobe injury is shown in Fig. 2. The group mean values are shown as either open or closed circles, or as crosses, depending upon the intensity level. The solid lines connect the mean CFF's for normal *S*s, the interrupted lines the values for patients with frontal lesions. These curves show how fusion frequency varies as a function of target area at each of the three intensity levels. In general, confirming the previous reports of Granit and Harper (**8**), and of Brown (**5**), CFF increases linearly with increase in log target

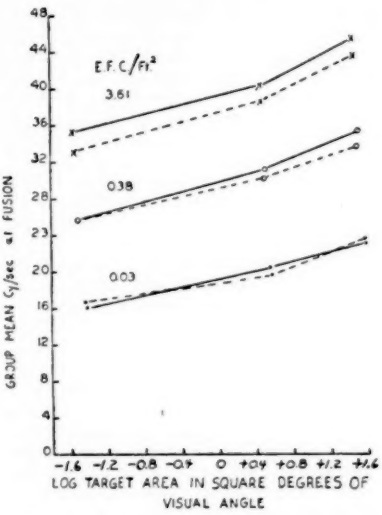

FIG. 2. Foveal CFF as a function of target area at fixed intensity levels. The solid and interrupted lines represent the data for normal *S*s and patients with frontal lobe lesions, respectively.

area. There is a tendency, however, for this area gradient to inflect upward at the fusion threshold for the largest target. This tendency is most marked at the higher intensity levels.

How CFF varies as a function of total light flux is not readily seen in Fig. 2, although the curves suggest that fusion is more affected by intensity than by target area. The effect of total light flux on CFF is more directly shown in Fig. 3. This graph relates CFF, on the ordinate, to log total light flux (in lumens/ft.²) along the abscissa; a steeply rising curve is obtained for each target size. These curves show that, at any one flux value, the smaller the target, the higher the CFF. Putting it another way, this means that the more the light energy is concentrated upon the fovea (since all targets were directly

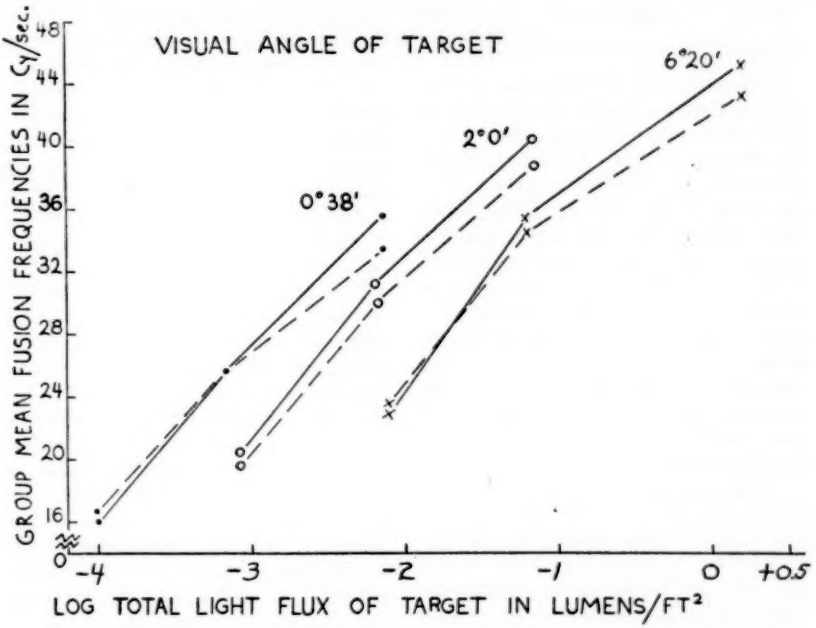

FIG. 3. Foveal CFF as a function of total light flux and target size. Group mean CFF, in cy/sec., along the ordinate; log total light flux, in lumens, on the abscissa. Target size, in visual angle, as indicated on each pair of curves. The solid and interrupted lines represent the data for normals *Ss* and patients with frontal lobe lesions, respectively.

fixated at their centers), the higher the fusion threshold.

A comparison of the solid and the interrupted lines in Figs. 2 and 3 shows that there is a tendency for the fusion threshold to be slightly lower for the patients with frontal lobe lesions, particularly at the higher intensity levels. In order to evaluate the significance of these differences between groups the *t*-test was utilized. The results of this statistical comparison are given in Table II, the sign of the mean difference indicating whether the pathological group was higher (+) or lower (−). These data show that the smallest target (38′), at the highest intensity level (3.61 e.f.c./ft.²), produced the only difference between the groups significant at the .05 level of confidence. The null hypothesis cannot be rejected.

TABLE II

DIFFERENCES AND *t*-RATIOS BETWEEN FRONTAL LOBE INJURED AND NORMAL GROUPS

Intensity in e.f.c./ft.²	Target Size in Visual Angle					
	0°38′		2°0′		6°20′	
	Diff.	*t*	Diff.	*t*	Diff.	*t*
3.61	−2.04	2.15*	−1.70	1.60	−1.77	1.54
0.38	−0.03	0.04	−1.03	1.30	−1.74	1.76
0.03	+0.67	1.43	−0.60	0.78	+0.38	0.48

* Significant at the .05 level.

Discussion

Effects of Frontal Lesions on CFF

The results of this experiment have shown that an injury to the frontal lobes does not necessarily result in a depression of the binocular fusion threshold for flickering light. This statement holds true for light intensities between .03 and 3.61 e.f.c./ft.^{2}, and for target sizes between 0°38′ and 6°20′ of visual angle. Since these intensity levels and target sizes cover a wide range, it would appear that these physical parameters are of little importance in demonstrating the depression of fusion frequency which has been reported by others for patients with frontal lesions.

From the results of our previous study (**2**), it would likewise appear that it is of no consequence whether monocular or binocular gaze is employed, or whether the flickering patch is in the center of the visual field, or in various peripheral positions. Under none of these conditions did we encounter a significant depression of CFF in patients with frontal lobe lesions. Other factors, therefore, must be involved in the reduction in CFF which has been reported (**9, 18**). These factors may be (a) special conditions of testing—conditions which have not yet been identified, or (b) differences in the populations studied.

Testing conditions.—So far, the differential effect of pulse shape, light-to-dark ratio, and light spectrum on normal and brain-injured individuals has not been investigated. Halstead used a strobotac (**9**), which produces brief light pulses of constant duration with an exponential decay. With this apparatus, the light-to-dark ratio necessarily varies with the frequency of the intermittent light. Under our own experimental conditions, the light-to-dark ratio remained constant at 1 and the pulses employed were either rectangular (**2**), or sinusoidal (**16, 17**). The spectral characteristics of flickering light have not been subjected to systematic variation in any study involving patients with brain injuries, but the light source in Halstead's study (**9**) was probably similar to that employed by others (**18**) and by ourselves (**2**).

Population differences.—Another factor to be considered is an actual difference in the populations studied. Halstead's (**9**) cases had undergone operations for the removal of brain tumors. The patients studied in our experiments had sustained cerebral gunshot wounds, either by bullet or shrapnel. Therefore, the nature of the pathology in the two studies was different; tumor represents a slowly developing cerebral lesion whereas gunshot wounds produce maximal damage at the time of injury or shortly thereafter. The presence of brain tumor (prior to surgery) in Halstead's patients may account for the apparent differences in the results of the two studies (**10**).

A final possibility is that a decreased fusion threshold occurs after any cerebral lesion, but is in part reversible; hence it cannot be detected if one begins testing 4-6 years after the trauma, as in the present study. This hypothesis receives some support if one studies the results recently published by Young (**18**). She used equipment and procedures similar to those of Halstead and reported no significant difference in CFF between patients with frontal lobe operations and non-operated control patients. However, she states that ". . . nine of the 17 operatees showed a decreased c.f.f. following operation. This decrease was most marked in those who had an originally high c.f.f. This

loss was greatest three weeks postoperatively, and was in several instances slightly regained four months postoperatively" (**18**, p. 263).

In order to analyze this statement, we have calculated the significance of the difference between preoperative and postoperative fusion thresholds, using the raw data published by Young (**18**, p. 262, Table 66). The results indicate a depression of 1.7 cy/sec. three weeks postoperatively, which is significant at better than the .01 level of confidence. At three months and one year after operation the depression is .6 and .8 cy/sec., respectively, neither value achieving significance at even the .10 level of confidence. These data suggest that a drop in CFF after frontal lesions might occur even in cases without tumor,[2] but that this drop may be transient. Moreover, the data do not preclude the possibility that similar transient reductions of CFF might appear after any cortical ablation, involving, for example, the temporal or parietal lobes. In the absence of information on this point, all that seems definite is the finding that lasting depressions of CFF appear in cases of involvement of the higher visual pathways (**2**, **14**, **15**, **16**, **17**).

Effects of Total Flux on CFF

The effects of total light flux on the fusion thresholds of the normal *S* are quite clear. For centrally fixated targets containing the same amount of total light flux, the smaller the target, the higher the CFF. This means, as we have pointed out, that flicker discrimination, over the range of intensities used, is optimal when the light energy is maximally concentrated upon the fovea.

On the basis of these findings it can be argued that what is called the "area effect" in flicker studies is actually a "total flux effect." That is, if target area is increased, while the intensity of the test light remains constant (in terms of e.f.c./ft.^{2}), then total flux (area $\times$ intensity) also increases. This results, as Fig. 3 shows, in an increase in CFF. On the other hand, if the target area is held constant and the intensity of the light is increased, the total flux again increases and so does the fusion threshold. The true "area effect" is encountered only if flux (total light energy) is held constant, but target area alone is varied. Under these conditions we find, for directly fixated targets, that the smaller the test patch, the greater the CFF. One must conclude therefore that fusion is influenced more by the density or concentration of the light energy in the center of the field, than by the amount of total light energy present.

Summary and Conclusions

This study was concerned with critical flicker frequency (CFF) as affected by (a) lesions in the frontal lobes, (b) target area, and (c) intensity of light source. Flicker was produced by means of an episcotister which interrupted a light beam focused upon a translucent screen. Measures of the foveal fusion threshold were obtained from 16 normal *S*s, and from 16 patients with known loss of frontal lobe tissue which was incurred 4 to 6 years prior to testing. Three target sizes (38′, 2°0′, and 6°20′ of visual angle), and three intensity levels (.03, .38, and 3.61 e.f.c./ft.^{2}) were employed. The following results were obtained:

[2] Young's cases had undergone topectomies, i.e., selective surgical removal of presumably healthy frontal cortex, for relief from psychosis.

1. To a first approximation, CFF increases linearly with increase in log target area. This finding confirms earlier observations (**5, 8**).

2. With the total light flux held constant, the smaller the area the higher the CFF of a directly fixated target. Thus, the more the light energy is concentrated upon the fovea, the higher the fusion threshold.

3. Frontal lobe lesions incurred 4 to 6 years prior to testing do not significantly depress CFF, regardless of target size or light intensity. This observation seems to contradict results previously obtained by others (**9**). Permanent reductions in CFF have been shown earlier to occur in patients with homonymous field defects (**2, 14, 15, 16, 17**). In view of these results it is proposed that transient depressions in CFF may occur after frontal lobe lesions (**18**), but that injuries to the higher visual pathways (retro-chiasmal) are necessary to produce a lasting reduction of CFF in man.

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SENSITIVITY TO APPARENT MOVEMENT IN DEPTH AS A FUNCTION OF "PROPERTY OF MOVEMENT" [1]

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A revival of interest in an empirical view of visual perception has taken place in the past few years. A number of investigators (**6, 8, 9, 10, 13**) have stressed the role of learning, past experience, the development of "assumptions," etc. in the perception of the visual world. For the most part, the data presented in these recent papers buttressing this point of view have been gathered from experimental situations requiring various kinds of size-distance judgments. In particular, three of the studies noted above (**8, 10, 13**) have utilized the phenomenon of apparent movement in depth as the medium for testing hypotheses. This particular phenomenon of movement is well known, and its history has been presented (**8**). It consists essentially of presenting a stimulus against a homogeneous background and continuously varying stimulus size. Optimal conditions for this kind of movement seem to be monocular viewing of a stimulus against a homogeneous black background. There exist no systematic data, however, for specifying optimal or minimal conditions for observing the phenomenon. It is not difficult nevertheless to arrange an experimental setup whereby *S* is convinced he is observing real movement. This comparability or perceptual identity of real and apparent movement has been stressed in various investigations of apparent movement, particularly in those dealing with "phi."

One aspect of movement, whether real or apparent, which has concerned some researchers has been the effect of certain non-stimulus factors, such as familiarity, meaning, etc. For example, it has been found that optimal movement (phi) occurs more readily with meaningful objects than with simple lines (**7**). Blug (**1**) also demonstrated that with drawings presented tachistoscopically, movement was facilitated for drawings carrying the suggestion of movement. Neff (**12**) has reported that the more nearly the object seen in movement has correspondence with everyday perceptions of moving objects, the greater will be the range of other stimulus variables. This is in line with the statement by Bourdon (**2**) that the illusion of movement is seldom seen in objects which are ordinarily immobile, e.g., rocks, houses. The notions above imply that one of the major determinants of movement is based upon a learning factor, or to put it in another way, that the sensitivity to real or apparent movement is enhanced when this sort of factor is present.

It was the purpose of the present experiment to test the hypothesis that the sensitivity to apparent movement in depth would vary with the nature of the stimulus when stimuli were classified on the basis of possessing the property of movement. The term "property of movement" was taken to mean that an object cannot be perceived apart from its functional significance. For example, the properties of a baseball are that it is round,

[1] This study was made possible by a grant from the Rockefeller Foundation to the Department of Psychology, Princeton University.

has seams, etc., and also that it is ordinarily seen as moving. It is this last factor which is designated as the property of movement. The question is whether the significance of this factor is of a sufficiently high order to affect the sensitivity to movement. The prediction was made that the sensitivity to apparent movement in depth would be greater for an object which in ordinary environments is perceived in motion (e.g., baseball) as compared to an object which would not fit this classification. Ordinary reaction time was used as the measure of sensitivity. The study was also designed to obtain data regarding varying conditions of observation for this kind of movement.

Apparatus and Stimulus Materials

In Fig. 1 is shown an isometric diagram of the apparatus used in the experiment. The main features of the apparatus are a control panel which *E* used to present and control experimental conditions (*A*); a Clason Projector[2] (*B*) for presenting the desired stimulus figure; a variable-speed planetary gear transmission[3] (*C*) for driving the movable lens element in the projector; a front surface mirror (*D*) mounted in a frame on top and at the front of the cubicle (*E*) from which *S* made observations; a viewing box (*F*) containing an evaporated metal film placed parallel to the surface of the front surface mirror above it; a screen (*G*) upon which was cast the desired image from the projector; and a binocular alley (*H*) containing various familiar objects at different distances.

The viewing box through which *S* observed consisted of ¼ in. plate glass top, front and back surfaces. The box was adjustable angularly by means of thumb screws. It could be adjusted vertically by moving the small platform upon which it rested. A cardbord mask placed on the top side of the box permitted monocular vision (right eye) of the stimulus figure. Thus when *S* looked into the viewing box, he saw the binocular alley through the evaporated metal film, but at the same time he saw with his right eye the twice-reflected image of the projected figure.

Within the cubicle an adjustable chin rest was mounted in front of the viewing box. Vertical side elements of this rest made it possible to apply light pressure to the temporal parts of the head in order to prevent lateral head movements. To the right and below the chin rest was mounted a telegraph key which *S* used as his response key.

Power from the variable speed transmission to the movable lens element in the projector was delivered through a worm and spur gear system. The use of the transmission permitted a wide range of possible drive speeds, but for purposes of the present study, a constant drive speed was employed. The electrical control system was so devised that when *E* threw a master switch, the projector bulb, the transmission and a Standard Electric Timer (1/100 sec.) were all started. Independent control of the projector light source was also possible. When *S* pressed the response key located inside the cubicle, these three elements were simultaneously stopped. By dropping a cardboard mask between the viewing box and *S*'s face, both the monocular and the binocular fields of view were cut off.

The binocular alley consisted of a long board 12 in. wide and 17 ft. long. The origin of the board was at the viewing box and extended straight ahead. This board, upon which were placed numerous familiar objects (e.g., cigarette packages, match boxes, soup cans, etc.), was illuminated by a number of small diffusion-type light sources. This illumination was about equally distributed throughout the binocular alley. The brightness of the alley measured from a piece of white cardboard with a Macbeth Illuminometer was .8 millilamberts. The brightness of the stimulus figures was approximately the same value.

The stimulus materials used in the experiment were six 35 mm. slides. Three different stimulus figures were made up in two sizes of slides. One figure was a baseball, one a plain white circular figure, and the third a white circular figure with randomly scattered spots and marks on it (con-

[2] This projector is a Bausch and Lomb device ordinarily used for testing visual acuity. Its unique feature is that it provides a means of continuously varying the angular size of a projected image and maintaining sharp focus. The projector uses 35 mm. slides. For purposes of the present study, the projector had one disadvantage. Its compensating lens element is cam driven and gives a non-linear size projection function for any constant drive speed. However, for approximately half of the projection range the rate of size-change is linear. It was within this range that the present study worked.

[3] Obtained commercially from Graham Transmission, Inc., Milwaukee, Wisconsin.

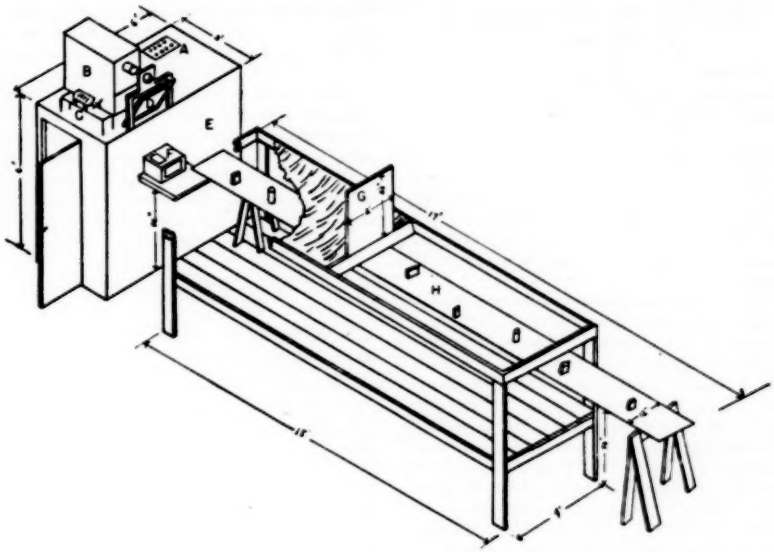

FIG. 1. Isometric diagram of apparatus: *A*—control panel; *B*—projector; *C*—transmission; *D*—front surface mirror; *E*—cubicle; *F*—viewing box; *G*—screen; *H*—binocular alley.

trol figure).[4] This third figure resembled the circular field of a microscope of an "active" water specimen. The three larger slides were the same size. The three smaller slides were also of equal size. The range of projected size (diameter) for the larger slides was 63 to 87 mm. The range for the smaller slides was 37 to 52 mm.

PROCEDURE

The task required of each *S* was quite simple. After being blindfolded, he was led into the large room containing the apparatus and then into the cubicle. After 5 min. of dark adaptation he was properly adjusted in the chin and head rest and *E* gave the following instructions:

> What we are interested in is how long it takes you to respond to the movement of an object which comes toward you or goes away from you. You are to watch the object which appears to the right of the alley and at the instant you definitely detect its movement, press the key. Remember, you are to press the key only at the instant you definitely detect movement. When I lift the mask, you will see the alley, and to the right of the alley, the object which you are to judge. I shall ask if you are ready. When you say "yes" I shall start the motor which controls the movement of the object. [The time interval between "yes" and the activation of the transmission was varied at random.] You should remember, and this is important, that just because you hear the motor start doesn't necessarily mean that the object will move. [This was true, for "false" trials were randomly distributed among the real trials by simply putting the transmission in neutral.] I shall then drop the mask and ask you to tell me in what direction the object moved, i.e., forward or backward. We shall then prepare for the next trial. Do you have any questions?

The required observations were then made. Prior to each observation *E* defined the particular stimulus which was to be judged. That is, *E* said one of three things on each trial; namely, "Here is the baseball; here is the white circle; here is the white circle with marks on it. We shall go ahead now." The stimuli were defined for *S* only in the first of the four sets of observation. After each trial, *E* would drop the mask, record the response time in 1/100-sec. units and prepare for the next trial. At the end of the experimental session, which required about one hour on the average, *S* was briefly interviewed regarding certain aspects of the judgments he made.

[4] This figure was incorporated in the design in order to insure that any differences obtained between the baseball and the plain white circle could not be interpreted as being due solely to the one having visual detail and the other not.

Subjects and Experimental Design

The design of the experiment was a systematic latin-square type involving 12 different arrangements of observations (3 stimuli, 4 conditions) for 12 *Ss*. The 12 arrangements were ordered at random and systematically rotated within the group of 12 *Ss* such that any one arrangement was given first to one *S*, second to another, third to another, etc. All *Ss* were college freshmen with 20/20 vision and were naive regarding the purpose of the experiment and the nature of the apparatus.

The three stimuli (baseball, plain circle, and control figure) have been described. The four observational conditions for these three stimuli involved two directions of apparent movement (forward and backward with respect to *S*) and two different rates of size-change of the projections. The directional change was accomplished by increasing (forward) or decreasing (backward) the size of the image. The rates of change were accomplished by using the two sizes of slides. The two rates of change with a constant excursion time of 13.20 sec. for the movable lens element were .88 mm. per sec. (small slides) and .55 mm. per sec. for the large slides. Table I gives the four conditions and their descriptions.

Each *S* made four observations on each of the 12 possible arrangements of stimulus conditions. This was done by presenting the set of 12 trials successively, allowing a brief rest period between sets. It was intended to treat the first set of trials as practice data, but subsequent analysis of these data revealed no systematic trends, so they were combined with the remaining data for the main analysis.

Particular restricting features of the apparatus required the inclusion of certain control trials in the experimental design. In addition to the control factors mentioned in the section on procedure, it was necessary to prevent *S* from realizing that the largest figure (to *S*, the nearest) observed (Cond. C) always receded and that the smallest figure (apparently most distant) always approached (Cond. B). This was done by randomly supplementing the regular trials by some presentations in which the figures under Cond. C increased in size rather than decreased, and by decreasing size in some presentations under Cond. B. These trials were disregarded in the analysis of the results.

TABLE I

Initial Sizes of Stimuli Before Movement, the Directions of Apparent Movement, and the Rates of Size-Change for the Four Conditions of Observation Investigated

Condition	Image Size (Diameter) in Mm. before Change of Size	Direction of Apparent Movement	Rate of Change of Size in Mm. per Sec.
A	52	Backward	.88
B	37	Forward	.88
C	87	Backward	.55
D	63	Forward	.55

Results

The response times to movement were recorded in 1/100 sec. units. Since each *S* repeated observations on each set of 12 stimulus arrangements, a mean response time was obtained for each of the arrangements. Table II gives the mean scores for each *S* under each condition.

It was suspected that the data would not be distributed normally. Consequently, a distribution of all individual response times making up the entries in Table II was plotted. The resulting plot indicated the need of an appropriate transformation, and this was done by transforming to two-place log units. The design of the experiment also presented a problem of analysis in that the sequence of observations involving the three stimuli and four conditions differed for each *S*. Consequently, in order to test this sequence effect an analysis of variance was made on a 12×12 latin-square, in which each cell was represented by the mean log time of four observations from the same *S*. The results of this analysis gave a mean square of .014 for sequence. Tested against a residual of .017, an *F*-ratio of 1.21 results which for 11 and 110 *df* respectively is not significant. With evidence that sequence as such would not be significantly disturbing to the further analysis of results, a second analysis of

TABLE II

MEAN RESPONSE TIME TO APPARENT MOVEMENT IN DEPTH FOR THREE DIFFERENT STIMULI UNDER FOUR CONDITIONS OF OBSERVATION

S	Cond.	Stimuli			S	Cond.	Stimuli		
		Ball	Plain Circle	Control Circle			Ball	Plain Circle	Control Circle
1	A	7.34*	12.39	12.03	7	A	6.92	9.55	8.10
	B	5.01	5.62	6.80		B	9.49	6.80	7.26
	C	3.80	5.24	7.55		C	5.47	4.50	5.46
	D	6.23	5.20	6.34		D	7.82	9.63	12.56
	Mean	5.60	7.11	8.18		Mean	7.42	7.62	8.34
2	A	2.10	2.60	2.76	8	A	4.12	6.98	6.05
	B	3.68	3.96	3.00		B	7.29	6.34	8.46
	C	1.95	2.20	1.57		C	4.11	4.85	4.62
	D	3.43	2.46	4.93		D	8.60	7.56	9.39
	Mean	2.79	2.80	3.06		Mean	6.03	6.43	7.13
3	A	5.41	7.58	6.45	9	A	3.88	7.73	5.18
	B	5.25	4.85	4.20		B	11.22	8.08	11.45
	C	3.52	3.79	6.66		C	5.45	4.22	4.62
	D	4.82	6.43	6.20		D	12.10	12.66	11.85
	Mean	4.75	5.66	5.88		Mean	8.16	8.17	8.28
4	A	6.04	9.67	7.44	10	A	2.98	3.61	3.79
	B	9.44	7.69	8.14		B	4.01	3.52	3.05
	C	5.99	6.23	6.60		C	3.08	2.90	2.84
	D	9.23	8.60	8.28		D	3.40	3.39	4.60
	Mean	7.68	8.05	7.62		Mean	3.37	3.36	3.57
5	A	2.32	4.12	7.02	11	A	5.64	12.90	9.70
	B	9.66	7.51	11.35		B	6.58	7.16	8.57
	C	2.06	2.98	2.40		C	5.18	5.36	6.29
	D	12.51	10.30	11.50		D	7.99	6.56	7.98
	Mean	6.64	6.23	8.07		Mean	6.35	8.00	8.14
6	A	5.84	8.62	8.30	12	A	5.08	7.16	8.56
	B	3.82	3.75	4.03		B	3.10	3.06	5.20
	C	4.43	4.89	3.75		C	2.58	4.12	3.79
	D	5.77	4.22	7.42		D	3.41	2.74	2.97
	Mean	4.96	5.37	5.88		Mean	3.54	4.27	5.13

* Each entry is the mean of four reactions, in .01-sec. units.

variance was made on the transformed raw data[5] with a triple classification breakdown: 3 stimuli, 4 conditions of movement, and 12 *S*s. The results of this analysis are given in Table III.

It can be seen from the last column in Table III that three of the six testable sources of variance are significant beyond the .001 level. First-order interactions were tested against the triple interaction. The fact that the two significant first-order interactions involved the main effect of condition made it impossible to test for this main effect directly by the usual procedures (5). For purposes of the present study, the analysis for this effect is given subsequently in the form of separate tests of significance.

[5] It is to be noted that the transformation was made upon each response time within each set of four trials per *S* and not directly upon the mean response times as given in Table II.

TABLE III

ANALYSIS OF VARIANCE OF LOG RESPONSE TIMES TO APPARENT MOVEMENT IN DEPTH

Source of Variance	Sum of Squares	*df*	Variance Estimate
Condition of Movement (4)	.895	3	.298
Stimuli (3)	.130	2	.065
Ss (12)	3.134	11	.285*
Interaction 1×2	.233	6	.039*
Interaction 1×3	1.661	33	.050*
Interaction 2×3	.071	22	.003
Triple Interaction: 1×2×3	.324	66	.005
Total	6.448	143	

* Significant at the .001 level.

The principal interest of this study was in the main effect of "stimuli," namely, to determine if the response times among the stimuli would vary significantly in favor of lower response times for the stimulus having the property of movement, e.g., the baseball. Testing the variance estimate for stimuli against the significant first-order interaction involving this effect, that is, condition-by-stimuli, gives an *F* ratio of 1.67 which is not significant. It is interesting to note that the subject-by-stimuli interaction is not significant when tested against the triple interaction term.

The second largest source of variance in the data is that resulting from *Ss*, and it is highly significant. Whether this represents real divergencies in sensitivity or differential utilization of the imposed criterion of judgment is not known, but it is obvious that among individuals the differences in response times to this kind of apparent movement are quite large.

It has been noted that the two significant first-order interactions involved the main effect of condition and that direct testing of this effect becomes impossible by the usual procedures of analysis. As the next best thing, a series of *t*-tests were run for the six possible combinations of conditions for each of the three stimulus figures. Of these 18 tests, eight of the differences were significant at the .05 level or better. Seven of these eight involved the comparison of some condition with Cond. C (that condition in which the apparent movement was backward, the initial angular size the largest, and the rate of change the slower of the two used). In each of these seven instances, Cond. C gave the fastest response time. It would seem, then, that any significance attached to the variance among conditions is for the most part due to the quicker response times associated with this one condition. This difference among conditions makes it difficult to analyze further the two factors involved in the four conditions, namely, direction of apparent movement and rate of size-change. The apparent backward movement was not consistently superior to the forward direction, and the fact that Cond. C involved the slower of the two rates of size-change indicates that the differences employed must have been below the differential threshold even though the order of difference was 1.6 to 1. The results regarding direction of movement and rate of size-change indicate the need for separate systematic study of these factors.

Supplementary to the quantitative results given above are the statements obtained in brief interviews given at the end of the experimental session. The *Ss* were queried about the purpose of the experiment and none indicated any relevant insight. Most remarks to this question centered around the possible differences between detecting movement forward or backward. Although some *Ss* discovered during the experiment that vision of

the figures was monocular, none suspected that the observed movement was apparent and not real. Questions relating to the relative ease of detecting movement for the various stimuli and for the conditions of observation were answered in widely different ways. For each *S* a comparison was made between the actual time data and the statements about the various observations and in general the agreement was not great, but for a few *S*s it was very marked, particularly with regard to differences among the four conditions. There was no general agreement among *S*s that one particular stimulus figure moved faster or was easier to detect.

One question asked of all *S*s related to the criteria used in making their judgments. It was expected that answers to this question might give indirect evidence bearing on the hypothesis tested. Three of the 12 *S*s were not able to state any basis for their judgments. The remaining nine *S*s, however, could be classified as using one of three criteria: (a) size, that is, three *S*s said they just waited until the figures *seemed* to grow bigger or smaller and then they could see the movement; (b) stimulus detail, that is, these *S*s said they watched the marks on the figures and when a change was noticed with respect to the whole figure, movement was seen (this criterion in so far as it applied to five *S*s confirmed the need for the control figure); (c) relation of the figure to the rest of the visual field, that is, *S* (one) attended to the apparent lateral distance between the figure and another object in the binocular alley—when this lateral relation was perceived as changing, movement was perceived.

Discussion

The results of the study do not bear out the prediction that the sensitivity to apparent depth movement is greater for figures which possess a property of movement. This lack of significant differences in response times among stimuli holds true overall for the four conditions tested. However, one condition (A) gave consistently faster response times for the figure defined as having the property of movement, i.e., the baseball. This is chiefly shown by the significant interaction between conditions and stimuli. It is not at all clear how this difference might be accounted for; but the presence of such interactions points at once to the vulnerability of such general hypotheses as that which initiated the present study. Studies of perception with a so-called empirical point of view have often been guilty of formulating hypotheses of a very general nature and using only one possible condition to substantiate a particular hypothesis. The results of the present study suggest that much would be gained by subjecting such general hypotheses to a multivariable design and analysis where possible significant interaction may be detected.

The quantitative results regarding the effect of stimuli possessing the property of movement are clarified to some extent by the reports gathered at the end of the experimental session. An analysis of *S*s' statements regarding how they made their judgments revealed that, in effect, three different criteria were used, none of which referred to the property of movement. All *S*s who were able to verbalize a basis for their judgments referred to some aspect of the stimulating conditions as the criterion for perceiving the movement.

The lack of significant effects with regard to stimulus differences in the present study is opposed by a number of studies and papers dealing with the problem of past experience, learning, and the perception of movement (**1**, **2**, **7**, **8**, **11**, **13**). However, the empirical data for kinds of movement other than "phi" are rather limited. In addition, the problem of control of stimulus conditions (angular size, etc.) is one which previous studies have not adequately met. Indeed, the problem of equating stimuli on every basis other than "meaning" is an almost insurmountable one. It is probably true that certain types of movement may show the effect of meaning, familiarity, etc., while others may not be so effected. Insofar as the necessary condition for apparent movement in depth is a continuous change in angular size, it would be expected that maximum differences in the perception of this movement would follow alterations in the peripheral conditions of stimulation. This would not necessarily be so, for example, with a phenomenon such as "phi." It should also be mentioned that the use of response time as a measure of sensitivity may not be delicate enough to show such effects, and that perhaps a more adequate test of the hypothesis would be the utilization of a straightforward psychophysical technique.

The finding of highly significant variance due to individuals is interesting, if not unexpected. This reflects, it would seem, the fact pointed out previously, namely, the use of different criteria of judgment by different individuals. The criteria used presumably differed considerably in the effectiveness with which they permitted the detection of movement. It is also interesting to note that for some *Ss* on some trials movement was not reported even though the maximum excursion of the size-change was made. This was characteristic of some *Ss* only. Whether this reflects simply a difference in criterion effectiveness or some kind of constancy phenomenon cannot yet be determined.

The results of the present study with regard to the four conditions of observation are in a way peculiar. The fact that Cond. C was the only one consistently superior to other conditions and was chiefly responsible for the variance among conditions is opposite to what might be expected. This condition presented the largest initial angular size and the direction of apparent movement was backward. On the basis of size alone one would expect this condition to give the slowest response time. In studies of real movement (**3**, **4**), sensitivity varies inversely with increasing size, i.e., a rough Weber function is obtained. A possible explanation for the faster response time to the largest figure concerns the effect of the phenomenal factor of apparent distance. When *Ss* were asked to state what stimuli and conditions seemed easiest to judge, five stated that the "nearest" figures seemed easiest. These responses suggest that the phenomenal characteristic of apparent distance may have been playing a part. Such characteristics have been found to be significant variables in studies of real movement (**3**).

Little can be said regarding the effect of direction and rate of change from the present data. The lack of consistent faster response times to the faster rate of change simply indicates that the difference in rates of change was below differential sensitivity. The factor of direction of movement for this kind of apparent movement needs to be studied systematically

under conditions where initial angular size can be held constant, which was not possible with the apparatus used in the present study.

Finally, it is of interest to note the significant interaction between *Ss* and the conditions of observation. The interaction was well borne out by the statements obtained from *Ss* regarding direction of movement. Each *S* was definite in stating that one direction or another was most easily judged. Whether such an interaction would hold up under less impoverished conditions is debatable, but this does not alter the desirability of using multivariable designs in testing hypotheses under restricted stimulating conditions. It is suspected that experimentation in visual perception which employs rather restricted viewing conditions (e.g., reduced stimulation) offers maximum opportunity for showing significant interactions, particularly those involving *Ss*. These interactions ought to be testable and known.

Summary and Conclusions

Using response time as the measure of sensitivity, the hypothesis was tested that sensitivity to apparent movement in depth would vary with the presence or absence of the property of movement characterizing a particular stimulus. The property of movement was defined as that characteristic of an object which describes its usual behavior pertaining to movement. The specific prediction made was that the sensitivity to movement of such a figure would be greater than for a figure not possessing such a characteristic. Certain other aspects of the phenomenon of apparent movement in depth were also investigated. It was concluded from the results that:

1. Tested over all conditions employed, the sensitivity to apparent movement in depth is not greater for stimuli characterized as possessing the property of movement. The major hypothesis was not confirmed.
2. The response time to apparent movement in depth varied as a function of different conditions of observations. It was found, however, that the only significant variation introduced into the data by condition was due principally to one condition involving the largest angular size and movement in the apparent backward direction. It was pointed out that this difference among conditions might reflect the operation of a phenomenal factor of apparent distance.
3. The further analysis of condition data pointed to the need of systematic study of the factors of direction of movement and rates of size-change for this kind of apparent movement.
4. Very wide differences were found among *Ss* in sensitivity to this kind of apparent movement. This finding was taken to indicate the use of quite different criteria by various *Ss*.
5. Significant interactions were discovered between conditions and stimuli, and also between conditions and *Ss*. It was suggested that one might usually expect significant interactions in visual perceptual studies utilizing impoverished stimulation, hence the need for multivariable designs to detect and test such interactions.

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